

Zacchaeus Revisited: Luke 19,8 as a Defense

The debate over the correct interpretation of δίδωμι and ἀποδίδωμι in Luke 19,8 continues⁽¹⁾. The text in question reads as follows:

σταθείς δὲ Ζακχαῖος εἶπεν πρὸς τὸν κύριον·
ἰδοὺ τὰ ἡμίσιά μου τῶν ὑπαρχόντων, κύριε,
τοῖς πτωχοῖς δίδωμι, καὶ εἴ τινός τι
ἔσυκοφάντησα ἀποδίδωμι τετραπλοῦν.

At issue is whether δίδωμι and ἀποδίδωμι should be understood as customary or futuristic presents⁽²⁾. The difference in interpretations substantially affects the meaning of the pericope. The customary present communicates Zacchaeus' usual attitude towards his possessions and how he conducts his business. As a rule, he gives half of what he has to the poor and restores fourfold anything he may have extorted from a person. The futuristic present, however, indicates Zacchaeus' resolve to adopt the stated attitude and conduct himself accordingly for the future.

The purpose of this article is to join the discussion of Luke 19,8 and suggest that, in the Zacchaeus story, Luke wanted to show how salvation came to a loyal Jew, a son of Abraham, without necessarily implying that Jesus saw him as a sinner. Like other stories in Luke-Acts, the Call of Levi, dinner at Simon's house, the Good Samaritan, the Prodigal Son, the Gamaliel speech, and the Phar-

(1) The most recent commentator is D. HAMM "Luke 19,8 Once Again: Does Zacchaeus Defend or Resolve?", *JBL* 107 (1988) 431-437, who offers a good summary of the discussion. Also see J. A. FITZMYER, *The Gospel According to Luke X-XXIV* (AB 28A; Garden City 1985) 1220-1221 for the major representatives of each position.

(2) The confusion over how the verbs should be taken is perhaps best illustrated by the fact that A. T. ROBERTSON, *A Grammar of the Greek New Testament* (New York 1914), refers to them as futuristic presents in one place (870) and customary presents in another (880). See N. M. WATSON, "Was Zacchaeus Really Reforming?", *ExpTim* 77 (1965-66) 283, and FITZMYER, *Luke X-XXIV*, 1221.

isees' defense of Paul, the Zacchaeus story explodes a stereotype and asks the reader to think the unthinkable. Jesus offers Zacchaeus salvation because he is a believing Jew and not because he had a sudden change of heart. His "sinner" label comes from the critical crowd, who echo a common view of toll collectors in Luke's day⁽³⁾. Thus, the interpretation favored here reads 19,8 as Zacchaeus' defense of his customary actions countering the false perceptions of his opponents, who are chiefly criticizing Jesus for associating with an apparent outcast. To interpret Zacchaeus' statement as a resolve is to read the story according to the stereotype Luke is trying to subvert.

The fact that Jesus vindicates Zacchaeus by pronouncing salvation for a "son of Abraham" is not incidental to the story. The presentation of Abraham in Hellenistic-Jewish and Christian sources, and the use of the patriarch in Luke-Acts provide additional information for reading Zacchaeus' claim as a defense. Thus Luke's linking of Zacchaeus to Abraham should receive more attention in this narrative than it usually gets. After addressing the issue of understanding 19,8 as a resolve or a defense, I shall discuss Zacchaeus as a legitimate heir to Abraham, and the recipient of salvation.

I. Defense or Resolve?

Although the weight of scholarly opinion favors understanding *ἰδῶμι* and *ἀποδίδῶμι* as futuristic presents, signalling Zacchaeus' resolve, there are compelling reasons to interpret these verbs as iterative, marking his customary actions⁽⁴⁾.

⁽³⁾ The extent to which toll collectors were seen as sinners is not at all clear. For an excellent treatment of the problem see J. R. DONAHUE, "Tax Collectors and Sinners: An Attempt at Identification", *CBQ* 33 (1971) 39-61.

⁽⁴⁾ See F. GODET, *Commentaire sur L'Évangile de Saint Luc*, vol. 2 (Paris 1889) 337-338; WATSON, "Zacchaeus", 283; and FITZMYER, *Luke X-XXIV*, 1221, the most recent commentator to adopt this view.

HAMM, "Luke 19,8 Once Again", 433, believes Fitzmyer's argument is the most cogent to date for reading 19,8 as defense rather than resolve. Its strength rests on the portrayal of Zacchaeus in the narrative. Zacchaeus' status as a repentant sinner is not clear in the story. Compared with other Lucan stories about sinners, Zacchaeus does not ask for mercy (cf. 17,13; 18,38) or claim any sorrow for sin (15,21; 18,13). Neither does Jesus refer to

Four difficulties are generally presented against reading Luke 19,8 as Zacchaeus' defense of his customary actions⁽⁵⁾. First,

his faith (cf. 7,50; 8,48), repentance or conversion (cf. 15,7,10). Jesus does not offer Zacchaeus forgiveness, but rather vindicates him. Thus the addition of v. 10 suggests that Zacchaeus had a claim to salvation, which Jesus came to offer him, regardless of the fact that he was a toll collector. Fitzmyer finds additional support for Luke's use of the customary present in 18,12 where ἀποδεκατῶ marks the Pharisee's understanding of his usual conduct.

(⁵) There are also some minor objections over exegetical matters. HAMM, "Luke 19,8 Once Again", 433-434, for example, questions the meaning of ὑπάρχοντα and ἐσυκοφάντησα in the context of a defense. He wonders how ὑπάρχοντα can regularly be halved and believes ἐσυκοφάντησα is an admission of past intentional injustices. But is this not a too narrow understanding of ὑπάρχοντα? In Luke 8,3 Joanna and Susanna, along with others, provide for Jesus' mission out of their means (ἐκ τῶν ὑπαρχόντων). Jesus exhorts his disciples to sell their possessions (ὑπάρχοντα) and to give alms in 12,33. This is extended to the renunciation of all that one has (πᾶσιν τοῖς ἑαυτοῦ ὑπάρχουσιν) at 14,33. In Acts 4,37 part of Barnabas' ὑπάρχοντα, a field, is readily converted to cash laid at the apostles' feet. These examples suggest that the meaning of ὑπάρχοντα could include income, either directly or as derived from property liquidated into cash. Though ὑπάρχοντα may primarily refer to property, it seems also to include income, which, if it can be disposed of for alms, etc. can easily be halved as a usual practice.

Ἐσυκοφάντησα in 19,8 need not be read as an indictment of Zacchaeus' past. When this verse is understood as a refutation of the charges made against Zacchaeus by those murmuring, its point is to show how different he is from the stereotypical toll collector, and why, as a rightful heir of Abraham, he is worthy of salvation. If those outside have wrongly perceived him and believe that he cannot have access to salvation simply because he is a toll collector, which they have made synonymous with sinner, then Luke explodes his readers' assumptions by showing just how righteous Zacchaeus is. He exceeds the requirements for restoration in Lev 6,5 (LXX 5,24), and Num 5,6-7. He conforms to those of Exod 21,37 and 2 Sam 12,6.

R. C. WHITE, "Vindication for Zacchaeus?", *ExpTim* 91 (1979-1980) 21, takes Zacchaeus' statement (v. 8) as one of defense against false charges, but does not understand his vindication to result from "Jesus' power, and his action on behalf of Zacchaeus, nor because of Zacchaeus' faith, but because Jesus believes him innocent, i.e. a true son of Abraham despite his job which branded him otherwise". But this is not completely accurate. The context of the story, following the teaching on the Day of the Son of Man (17,22-37) and the parable of the Unjust Judge (18,1-8), coupled with Zacchaeus' humble stance before Jesus argue in favor of Jesus' power to vindicate him. Also Jesus' action on behalf of Zacchaeus brings the promises of Abraham to fulfillment for him.

anything but a resolve on Zacchaeus' part would be inappropriate in the story, largely because a defense at this point would turn him into a boaster⁽⁶⁾. Second, "salvation" (σωτηρία) in v.9 should be understood within the larger context of Luke's Gospel, and the meaning of "lost" (ἀπολλός) in v.10 should be chiefly informed by the parables in chapter 15, which feature the sinner who is found, i.e., repents⁽⁷⁾. Third, the wider perspective of Luke's Gospel supports the interpretation of 19,8 as Zacchaeus' post-conversion resolve, and the Zacchaeus story should be read as a parallel to the story of Levi (5,27-32) and the lost and found parables of chap. 15⁽⁸⁾. Fourth, a defense would render Jesus superfluous to the narrative⁽⁹⁾. About these objections I make the following observations:

WATSON, "Zacchaeus", 283, understands ἐσυκοφάντησα to mean deliberate acts of extortion and asks "But is it likely that a man guilty of such acts would be so conscientious as habitually to make fourfold restitution". He joins PLUMMER, *The Gospel According to Luke*, 435, in this interpretation. To answer his question: Probably not. But if the extortion were not deliberate, Zacchaeus, when he discovered it, could plausibly make an over-generous restoration as just compensation. Could not his generosity, then, argue in favor of ἐσυκοφάντησα as inadvertent extortion? Therefore ἀποδίδωμι need not signal restoration for past offenses, it could just as easily point to Zacchaeus' righteousness, as his name implies (FITZMYER, *Luke X-XXIV*, 1223). WHITE, "Vindication for Zaccheus?", 21, finds implicit irony in the name.

⁽⁶⁾ HAMM, "Luke 19,8 Once Again", 435; M.-J. LAGRANGE, *Évangile selon Saint Luc* (Paris 1927) 489; I.H. MARSHALL, *The Gospel of Luke* (NIGTC; Grand Rapids 1979) 698; A. PLUMMER, *The Gospel According to Luke* (ICC; Edinburgh 1908) 435; J. SCHMID, *Das Evangelium nach Lukas* (RNT; Regensburg 1960) 287.

⁽⁷⁾ HAMM, "Luke 19,8 Once Again", 435-436, believes the associations of "lost/found", "knowing salvation/sinner repenting" anticipate the Zacchaeus story and provide the background against which one should view Zacchaeus, once lost now found by the Son of Man. See also W. LOWE, "Towards an Interpretation of Luke 19:1-10", *CBQ* 36 (1974) 321-333; J. O'HANLON, "The Story of Zacchaeus and the Lukan Ethic", *JSNT* 12 (1981) 19; and J. ERNST, *Das Evangelium nach Lukas* (RNT; Regensburg 1977) 512-513.

⁽⁸⁾ HAMM, "Luke 19,8 Once Again", 436-437; ERNST, *Das Evangelium nach Lukas*, 513; SCHMID, *Das Evangelium nach Lukas*, 287.

⁽⁹⁾ R.J. KARRIS, "The Gospel of Luke", *The New Jerome Biblical Commentary* (Englewood Cliffs, NJ 1990) 711.

First, the propriety of Zacchaeus' claim should be decided by the context of his words. If he is not making a resolve, why must Zacchaeus be boasting rather than defending himself against those who have wrongly accused him? Admittedly, the story is scarce on details, but it is important to remember that some people outside murmured a charge against Jesus. It is one that others have murmured elsewhere in Luke (5,30; 15,2). The complaint implies that, because Zacchaeus is a toll collector, he is a sinner. Sharing hospitality with him then places Jesus in the position of associating with someone they consider an outcast.

"Murmuring" in Luke-Acts carries the paradox of seeming to be secret and quiet, while having the effect of a public manifestation. It always implies a controversy and calls for defense or resolution. The sequence of a story about "murmuring" shows the report of the murmuring, a direct statement of the charge, or a summary of what is at issue, and then the statement of defense, or the resolution of the conflict. A look at the Levi story (Luke 5,29-32) is instructive.

In that story the murmuring is against Jesus' disciples, but the charge is levelled at Jesus alone in the form of a question, "Why do you (pl.) eat and drink with tax collectors and sinners?" (RSV). Jesus then speaks directly to the complaint made against him (as well as Levi and the others by implication), clearing himself and them.

In the Zacchaeus story, the charge is made against Jesus (and Zacchaeus by implication), but Zacchaeus speaks directly to the complaint clearing himself and Jesus. Then Luke directs Jesus' words to Zacchaeus, and the fact that his remark follows on Zacchaeus' claim makes it a response. But Jesus' reference to Zacchaeus in the third person in v. 9 shows that his response is intended also for the murmuring crowd⁽¹⁰⁾. This double duty for v. 9 suggests that Zaccheus' claim in v. 8 does the same. Indeed, he directs his remark to Jesus, but does it for the benefit of the murmuring crowd too. In light of their complaint a defense of his customary action is quite appropriate. Therefore, turning Zacchaeus' statement in v. 8 into a boast would remove a necessary element of a story involving

⁽¹⁰⁾ MARSHALL, *The Gospel of Luke*, 698, suggests the possibility that πρὸς αὐτόν (v. 9) may be translated "about him" on an analogy with 18,9 and 20,19. See also FITZMYER, *Luke X-XXIV*, 1225 and GODET, *Luc*, 338-339.

“murmuring” against Jesus or others associated with him: the defense.

But Hamm, for example, argues that Zacchaeus could be seen as boasting, because an apology in the context of the participial form *σταθείς* has to be considered public⁽¹¹⁾. What makes the apology public, however, is not the flag *σταθείς*, but the occasion that requires it⁽¹²⁾. If Zacchaeus were aware of what the crowd murmured, then his statement in v.8 would more naturally be read as a public defense in response to what they have said.

The story’s dynamic, playing the implicit off the explicit to produce mutual vindication, supports this interpretation. In form it is a pronouncement story which leads to the point that Jesus’ mission is to save outcasts. The narrative setting and dialogue establish the conflict which the pronouncement seeks to resolve. After the preliminaries of character, place and event are fixed, Jesus is explicitly criticized by the murmuring people for going to Zacchaeus’ house (v.7). The implication is that Jesus is a sinner for associating with someone perceived to be a sinner. Zacchaeus then explicitly defends himself and, by implication, defends Jesus’ action (v.8). This has the effect of neutralizing the crowd’s criticism. Finally, the story ends with Jesus’ explicit vindication of Zacchaeus (v.9) and an implicit justification for Jesus’ action in the pronouncement of v.10.

If Zacchaeus were stating his resolve, those murmuring would be justified in their criticism of Jesus, and the effect of the final

(¹¹) “Luke 19,8 Once Again”, 435. Why Zacchaeus’ knowledge of the crowd’s murmuring renders his claim as a boast is not clear, but Hamm believes such knowledge confirms PLUMMER’S observation, *The Gospel According to Luke*, 435, that to read v.8 as a defense is to turn him into a boaster. To what Hamm says about the comparison with the Levi story in chap.5 I would add that the implication that Zacchaeus knows what they said makes as much sense as the implication that Jesus knows what the Pharisees and scribes said about him in 15,2, or the report in Acts 6,1 that the Twelve knew what the Hellenists murmured against the Hebrews without getting a direct report about the charges from anyone.

(¹²) On this point the evidence is actually divided. HAMM, “Luke 19,8 Once Again”, 435, cites four instances in Acts where *σταθείς* signals a public pronouncement: 2,14; 5,20; 17,22 and 27,21. But there are as many other places where the usage does not mark a public pronouncement: Luke 18,11; 18,40; Acts 11,13 and 25,18. WHITE, “Vindication for Zacchaeus?”, 21, reads *σταθείς* as indicative of “a defensive, self-assertive manner”.

pronouncement would be undercut. But it must be remembered that in Luke the murmuring against Jesus and those associated with him is never justified⁽¹³⁾. In the end, then, Jesus' pronouncement achieves two purposes: it immediately vindicates Zacchaeus and also clears himself of any wrongdoing. For the story's dynamic to succeed and for Jesus' pronouncement (v.10) to function properly, Zacchaeus' statement in v.8 has to be read as a defense rather than as a resolve, and that apology need not be a boast.

Second, "salvation" (σωτηρία) in Luke has more meanings than just what is brought about by the forgiveness of sins, as in 1,77⁽¹⁴⁾. It also refers to the Davidic Messiah in 1,69 and rescue

(13) J. B. TYSON, "The Jewish Public in Luke-Acts", *NTS* 30 (1984) 577, points out that the crowd in Luke-Acts does not function as a Greek chorus because its judgment is never appropriate. The crowd in 19,7 makes a wrong judgment about Jesus and Zacchaeus.

(14) Some might object that Jesus' mission in Luke is primarily concerned with the forgiveness of sins, rather than with the vindication of outcasts. I do not contest that Luke stresses the forgiveness of sins as an important effect of salvation. But it is interesting that it is absent from what many consider to be the programmatic statement of Jesus' mission in 4,18-19. Although it first appears as part of Jesus' work in 5,20, it is not formally a part of his mission until the pronouncement in 5,32. Yet he begins to fulfill his mission immediately when he leaves the synagogue in Nazareth by performing a series of healings where there is no mention of the forgiveness of sins (4,31-41; 5,12-16). Even in the scene where Simon Peter is called to discipleship, when he protests that he is a sinful man, Jesus says nothing about forgiving his sins (5,1-11). After 5,20 Jesus performs healings without reference to the forgiveness of sins (6,17-19; 7,1-23; 8,26-32). Interestingly, the Beatitudes and Woes (6,20-26) have no reference to the forgiveness of sins, and when Jesus commissions the Twelve in 9,1-6 he empowers them to cast out demons, cure illnesses, preach the Kingdom of God and heal, but he does not instruct them to forgive sins. Such healings, exorcisms, calls etc., are usually associated with salvation in Luke-Acts. But just from these few examples of important material from his Gospel one might conclude that Luke did not always associate salvation with the forgiveness of sins.

One example, which may shed light on the Zacchaeus story, is the account of the cleansing of ten lepers in 17,11-19. Like the Zacchaeus story it too explodes a stereotype. Zacchaeus was interested in Jesus and made an effort to see him (ἐζήτει ἰδεῖν, 19,3) as he entered Jericho. Although he is a toll collector, he observes the requirements of the Law, and is vindicated by Jesus. The lepers cried out in a loud voice to get Jesus' attention. The Samaritan among them, after having performed the requirements of the Law, had his eyes opened (ἰδὼν ὅτι ἰάθη, 17,15) to who Jesus was and

from one's enemies in 1,71, neither of which have to do with sin and repentance⁽¹⁵⁾. One must remember too that Luke uses the neuter σωτήριον to refer to the salvation of Jew and Gentile alike in 2,30, and that of all flesh in 3,6. Then, when one takes into account the use of σωτηρία in Acts (4,12; 7,25; 13,26,47; 16,17), where in one instance it even refers metaphorically to rescue under the image of saving hair from falling out (27,34), the fact that it means more than just what is offered to repentant sinners is clear⁽¹⁶⁾.

The suggestion that, when σωτηρία is understood within the wider context of Luke, Zacchaeus must be seen as a repentant sinner experiencing conversion is not necessarily helpful for determining the meaning of the word in 19,9. Since the story is lacking all the links to sin and repentance, if anything, the wider context of Luke-Acts would suggest that the σωτηρία offered to Zacchaeus is not contingent on his being a sinner.

When the meaning of "lost" (ἀπολωλός) is determined chiefly by its use in chap. 15, then the "seeking and saving" of the "lost" in 19,10 appears to relate to sin and repentance. The analogy to sheep, coin and son lost and found argues for that conclusion, and the function of the parables ensures that interpretation. In the absence of the language of sin and repentance, is not the function of the pronouncement in 19,10 different from the proclamation of salvation to a repentant sinner? The previous verse indicates that Zacchaeus has received salvation as a son of Abraham, which would suggest that he represents Israel for whom the offer of salvation is still open in Luke-Acts⁽¹⁷⁾. According to Fitzmyer, the allusions

returned to give thanks. Jesus pronounces salvation for him not because of sins forgiven, or because he was healed, but rather due to his faith (ἡ πίστις σου σέσωκέν σε, 17,19). I am grateful to J. A. Fitzmyer for bringing the story of the ten lepers to my attention.

(15) HAMM, "Luke 19,8 Once Again", 435, agrees that there are four uses of σωτηρία in Luke.

(16) See J. A. FITZMYER, *The Gospel According to Luke I-IX* (AB 28; Garden City 1981) 222-223, who is correct in the range of meanings Luke provides for σωτηρία in his gospel. He aptly catches the function of 19,10 as applicable to more than just the case of Zacchaeus when he writes: "'Salvation' is best summed up in one of Jesus' sayings preserved in 19,10: 'The Son of Man has come to seek out and save what was lost'" (223).

(17) J. DUPONT, *The Salvation of the Gentiles: Studies in the Acts of the Apostles* (New York 1979) 11-33.

here to the seeking and saving in Ezekiel 34 are clear⁽¹⁸⁾. As a contrast to the Pharisees, the role of Jesus is much enlightened by this text which chastises the shepherds of Israel who have not done their job (Ezek 34,3-5). What God promises to do for Israel is not unlike the mission of Jesus described in Luke 7,22. Not every biblical reference to a lost sheep has to point to a sinner.

Third, a closer examination of Luke 5,27-32; 15,1-32 and 19,1-10 will show some important differences. 1) Pharisees and scribes are not mentioned in the Zacchaeus story. 2) Table fellowship appears only in the story about Levi. Chap. 15 has toll collectors and sinners drawing near to Jesus, and the Pharisees and scribes murmuring and saying, "This man receives sinners and eats with them" (v. 2). Although it is probably implied, the Zacchaeus story is not primarily about table fellowship, does not mention it, and rather has to do with hospitality. 3) While both the Levi story and the application of the parables of chap. 15 mention *μετάνοια*, the Zacchaeus story does not. It talks about salvation (*σωτηρία*, *σῶσαι*) which the other two blocks of material do not. 4) The parables in chap. 15 talk about losing and finding, something absent in the Levi story, and link that to the repentance of sinners. The Zacchaeus story simply talks about saving the lost, without mention of sin or repentance. 5) The element of rejoicing is strongest in chap. 15. There is mention of it in the Zacchaeus story, and it is absent in the story of Levi. More importantly, it functions differently in these two blocks of material. In chap. 15 rejoicing is the attitude of the shepherd who finds the sheep (v. 5), the woman who finds the coin (v. 9), those who celebrate with each of them (vv. 6, 9), heaven (v. 7) and the angels of God (v. 10). It seems not to mark the attitude of the repentant sinner⁽¹⁹⁾. In the Zacchaeus story, however, rejoicing marks the chief toll collector's attitude of hospitality in receiving Jesus into his house (19,6).

Whereas there may be some similar vocabulary and themes among these three blocks of material, there are significant dif-

⁽¹⁸⁾ *Luke X-XXIV*, 1222, 1226.

⁽¹⁹⁾ Actually of all the references to joy and rejoicing in Luke-Acts only one may mention rejoicing in a conversion story. After his baptism the Ethiopian eunuch goes on his way rejoicing (Acts 8,39). The text, however, is not clear, for *ἐπορεύετο γὰρ τὴν ὁδὸν αὐτοῦ χαίρων* at the end of the verse could refer to Philip, who went on his way rejoicing.

ferences. Lining up their similarities without regard to the context and function of details in each does little to resolve the problem of 19,8. In fact, the story of Levi and the material of chap. 15 have more in common with each other than either or both of them have with the Zacchaeus story. Luke 19,1-10 should be treated differently, for it really is not about repentance, but is rather about Zacchaeus' salvation seen as the fulfillment of the promises made to Abraham.

Fourth, Jesus need not be rendered superfluous to the narrative, should Zacchaeus be defending his customary action before his accusers. Luke's challenge of a facile connection between toll collectors and sinners gives Jesus a critical role in the narrative, as Zacchaeus' vindicator (savior). Zacchaeus' defense undermines his critics by showing that his customary action is marked by honesty, but without Jesus' vindication his claim would be ineffective. The key verses are 19,9-10 where Jesus pronounces salvation for Zacchaeus as a rightful heir to Abraham, despite the way others might see him, and legitimates his own mission to apparent outcasts. Regardless of what he says about himself he stands in need, for without Jesus he is incapable of making an adequate defense of his life⁽²⁰⁾.

Whereas the four main objections to reading Luke 19,8 as a defense raise interesting exegetical issues, they are finally unconvincing for settling the matter. Addressing those objections has shown why there are good reasons for interpreting *δίδωμι* and *ἀποδίδωμι* as iteratives, and for understanding Zacchaeus' claim as a defense rather than a resolve. As with commentators who take the opposite view, I too believe that the Zacchaeus story should be read in context, paying close attention to the story's details in their immediate context before looking for their meaning elsewhere in the Gospel. To that end Luke 19,1-10 should be understood as a story about Zacchaeus, a rich toll collector, a Jew to whom Jesus came to bring salvation because he was a son of Abraham.

⁽²⁰⁾ J. B. GREEN, "Jesus and a Daughter of Abraham (Luke 13:10-17): Test Case for a Lucan Perspective on Jesus' Miracles", *CBQ* 51 (1989) 651, independently concludes about the daughter of Abraham in Luke 13,16 to a point similar to mine about Zacchaeus, as a son of Abraham: "persons thus denoted are people in need of God's mercy, persons defined by others as existing outside the boundaries of God's chosen, yet the very people to whom God shows his fidelity and brings salvation". I am grateful to J. P. M. Walsh for showing me this article in proof form.

Why was this important to Luke? Luke had at least two related purposes in telling the Zacchaeus story. They are obscured because he has loaded the story with images and ideas that reflect some of his main interests in other sections of the Gospel⁽²¹⁾. In part, he wanted to challenge the view that Christianity in his day should not be associated with the rich, with toll collectors, or with certain pious Jews simply because they were members of those groups. Basically it is a matter of who can be saved, and Luke wanted to show that anyone who fulfills the mandate of the gospel can be saved, regardless of his or her status. In the case of Zacchaeus he seems to be answering the question whether a pious Jew, who is also a chief toll collector, can have material possessions and the associations his business requires, and still be saved.

Another purpose of this story may have been apologetic, to speak to the question of the inclusion of Jews in the universal salvation that Luke understood Jesus to have brought about. In this way the example of Zacchaeus represents the fulfillment of the promises made to Abraham that figure prominently in predictions in the Infancy Narrative (1,54-55.70-75)⁽²²⁾. Thus the Zacchaeus story may have been intended, in part, by Luke to speak to his community's mission to Jews.

Zacchaeus has access to salvation not by his status alone as a rich toll collector, or a Jew, but by the fact that he is also a son of Abraham⁽²³⁾. For that reason Luke's casting Zacchaeus as a

⁽²¹⁾ See LOWE, "Towards an Interpretation of Luke 19:1-10", 329 and O'HANLON, "The Story of Zacchaeus", 9-10.

⁽²²⁾ The story about the woman who is a "daughter of Abraham" (13,10-17), also used to confound Jesus' critics, seems to function in like manner, as does the figure of Lazarus (16,19-31). It is interesting to note that neither of these stories, dealing with the effects of salvation, talks about repentance and the forgiveness of sins. J.D.M. DERRETT, *Law in the New Testament* (London 1970) 78-99, connects Lazarus with Eliezer in midrashic tales, who as Abraham's servant (Gen 15,2) was supposed to spy on the descendants of Abraham and then inform the patriarch of how well they kept the Law, especially regarding almsgiving to the poor and hospitality to strangers; cf. J.R. DONAHUE, *The Gospel in Parable* (Philadelphia 1988) 169-170.

⁽²³⁾ J.T. SANDERS, *The Jews in Luke-Acts* (Philadelphia 1987) 62-63, tries to connect "Son of Abraham" in Luke 19,9 to Gal 3,7 and make Zacchaeus into a Christian. He also suspects that Zacchaeus, as an outcast, is a prototype of a Gentile. See A. LOISY, *L'Évangile selon Luc* (Paris 1924) 457. FITZMYER, *The Gospel of Luke X-XXIV*, 1221, argues against this sense of "spiritual sonship" for Zacchaeus and is correct to see him as a Jew.

genuine son of Abraham is significant. Jesus' association with him is not what his critics think it is, because Zacchaeus is much more than they perceive. In Luke's eyes this distinguishes him among Jews, for not everyone of them responded to the offer of salvation made in Jesus. Some did, however, and in his day continued to do so. For those, who, like Zacchaeus, are true children of Abraham the promises made to the patriarch are still being fulfilled.

What follows is an examination of the character of Zacchaeus as a son of Abraham and the context of this story in Luke's Gospel. This will provide additional information for understanding Luke 19,8 as a defense rather than as a resolve.

II. Zacchaeus as a Son of Abraham

John Drury has posited an interesting origin for Luke's story of Zacchaeus. Noticing the location of the story in Jericho, the identification of the names Jesus and Joshua, the questionable occupation of Rahab, who gave the first Joshua lodging there, her venerated place in Christian tradition (Jas 2,25), and the association of toll collectors and harlots in Matt 21,31-32, he concludes that Luke transformed the figure of Rahab into her male counterpart, a toll collector, Zacchaeus⁽²⁴⁾.

John O'Hanlon criticized Drury's analysis as too speculative and hypothetical and raised many good questions about its methodology and content⁽²⁵⁾. But Drury may not be completely off the mark in seeking a connection between Zacchaeus and Rahab. His argument can be strengthened by four pieces of additional evidence which provide a tentative link between them. First, both Josh 2,1 and Luke 19,1 use the designation τὴν Ἱεριχώ for the location. Second, Josh 2,1 identifies Rahab as a harlot by the expression εἰς οἰκίαν γυναικὸς πόρνῃς, ἥ ὄνομα Ῥααβ, and Luke 19,1 identifies its subject as an ἀνὴρ ὀνόματι καλούμενος Ζακχαῖος, καὶ αὐτὸς ἦν ἀρχιτελώνης. Third, to describe Zacchaeus' hospitality, Luke 19,7 has καταλύειν, the same verb the LXX uses to report Rahab's lodging of Joshua's two men (Josh 2,2). Fourth, Jas 2,25 has

⁽²⁴⁾ *Tradition and Design in Luke's Gospel: A Study in Early Christian Historiography* (London 1976) 73-74.

⁽²⁵⁾ "The Story of Zacchaeus", 6-9.

ὕποδέχομαι to characterize Rahab's welcome of the messengers (ἄγγέλους). This is the verb Luke uses to describe Zacchaeus' welcome of Jesus (19,6). As Drury noted, Luke and James are the only New Testament authors to use ὑποδέχομαι⁽²⁶⁾.

Allusions to Rahab seem then to be part of Luke's story of Zacchaeus, and while Drury's connection between Zacchaeus and Rahab is enhanced by the above-cited evidence, his explanation of the connection is faulty. Rahab is important for Luke's presentation of Zacchaeus not because she was a harlot, but because she was a model of hospitality.

Luke may have thought of Rahab in relation to Zacchaeus because of his familiarity with the tradition about her attested to in Jas 2,18-25. There she exemplifies justification by works and is presented as a model of hospitality in that regard. But she is not the only one featured in that text. The example of Abraham precedes her as one who was also justified by his works, principally his willingness to sacrifice his son.

That text is peculiar for not mentioning Abraham's legendary hospitality⁽²⁷⁾. Roy B. Ward offered a way out of this dilemma when he proposed two reasons why Abraham's hospitality was presupposed there: first, because ἔργα in 2,21 is plural and only a singular example, the binding of Isaac, follows and second, because his hospitality and the testing of his faith in the binding of Isaac are frequently tied together in Jewish and Christian tradition. He cites as well GenR 56.5 and 54.4 where the sparing of Isaac is a reward for Abraham's mercy, especially his hospitality. He notes also that in 1 Clem. 10,10,12 Abraham and Rahab both appear as examples of hospitality. Therefore Abraham's hospitality should also be considered as one of his works⁽²⁸⁾.

The Christian tradition of justification by works in Jas 2,18-25, joining the examples of Abraham and Rahab, provided Luke with a source for portraying the chief toll collector. In that tradition he found examples of Jews who were justified by what they did, for

⁽²⁶⁾ *Tradition and Design in Luke's Gospel*, 72.

⁽²⁷⁾ On the tradition of Abraham's hospitality in Hellenistic Judaism, see S. SANDMEL, *Philo's Place in Judaism: A Study in the Conception of Abraham in Jewish Literature* (New York 1971) 83-85.

⁽²⁸⁾ "The Works of Abraham: James 2,14-26", *HTR* 61 (1968) 286-289.

their good works. But because of his primary interest in casting Zacchaeus as a true descendant of Abraham, what points of contact there are between Zacchaeus and Rahab are meant to supplement those between Zacchaeus and the patriarch. The connection between Abraham and Zacchaeus does not end there.

A growing body of literature argues for an adjustment in Luke's understanding of Jews and their place in the Christian mission⁽²⁹⁾. Previous views stressing the rejection of Jesus by Jews and the subsequent acceptance by Gentiles do not adequately represent the complexity of this issue in Luke-Acts⁽³⁰⁾. A consensus is emerging that Luke saw a division within Judaism itself between believing and non-believing Jews. This division, as found in Luke-Acts, may reflect the Lucan community's attempt to deal with the issue of an on-going Jewish mission⁽³¹⁾. His understanding of the validity and efficacy of the promises made to Abraham offered Luke a way of fitting believing Jews into the picture of universal salvation that dominates his soteriology.

Nils A. Dahl has shown that the importance of the Abraham story in Luke-Acts lay in the fact that Luke believed the promises made to Abraham were still operative in Jesus' time, and in his own⁽³²⁾. Dahl's analysis of the use of the Abraham story in Luke-Acts is accurate. Abraham does function as a Jewish patriarch

⁽²⁹⁾ See R.L. BRAWLEY, *Luke-Acts and the Jews: Conflict, Apology, and Conciliation* (SBLMS 33; Atlanta, GA 1987) 3-4.

⁽³⁰⁾ SANDERS, *The Jews in Luke-Acts*, 39-42, summarizes these earlier views and their contemporary counterparts.

⁽³¹⁾ P. ESLER, *Community and Gospel in Luke-Acts: The Social and Political Motivations of Lucan Theology* (Cambridge 1987) 31, has suggested that the Lucan community was made up of a mixture of Jews and Gentiles, the majority of whom had not converted from idolatry, but had come out of Jewish synagogues. He sees the issue of table fellowship between Jews and Gentiles as "a matter of intense controversy" (71) within the Lucan community. N.A. DAHL hints at the same in "The Purpose of Luke-Acts", *Jesus in the Memory of the Early Church* (Minneapolis 1976) 96-98. See also R.J. KARRIS, "Missionary Communities: A New Paradigm for the Study of Luke-Acts", *CBQ* 41 (1979) 80-97, esp. 90-97.

⁽³²⁾ "The Story of Abraham in Luke-Acts", *Studies in Luke-Acts* (ed. L.E. KECK - J.L. MARTYN) (Philadelphia 1980) 139-158. Following the thought of SANDMEL, *Philo's Place in Judaism*, 29, "To see what the writer makes of Abraham is often to see most clearly what the writer is trying to say", Dahl's aim was to show that the use of the Abraham story is telling for understanding the message and meaning of Luke-Acts. While Dahl

in these volumes, and has less to do with Christian believers. To that extent he fits with Luke's purpose of proclaiming universal salvation, and he is an important figure in the balancing act Luke does to maintain equal access to Christianity for Jews and Gentiles alike. Confidence in this belief contributed to his nuanced understanding of Judaism in the first century CE, one that respected the fact that some Jews believed in Jesus, whereas others did not. The former are recipients of the promises made to Abraham, now come to completion in Jesus⁽³³⁾. This view of how Judaism related to Christianity afforded Luke opportunities to show the fidelity of God's promises to Jews who stand outside the circles of Pharisaic and Sadducean Judaism, those in his gospel who are examples of Jews who reject Jesus⁽³⁴⁾.

The use of Abraham traditions, then, fits with Luke's penchant for a prophecy and fulfillment motif in his two volumes⁽³⁵⁾. Key references to the promises made to Abraham appear to indicate how Luke sees those promises still in effect and coming to fruition for Jews in Christianity⁽³⁶⁾. Thus Dahl is right about how Luke

believes Abraham remains the father of Jews and is not the father of Christian believers in Luke-Acts, he does not see him as a model to be imitated. Rather his function in Luke-Acts is to make a connection showing the continuity between Abraham's history and the events Luke writes about (140). L. T. JOHNSON, *The Literary Function of Possessions in Luke-Acts* (SBLDS 39; Missoula, MT 1977) 121-122, locates the beginning of Luke's interest in the promises of Abraham in the Gospel's prologue when it refers to τῶν πεπληροφορημένων ἐν ἡγῶν πραγμάτων, "the things brought to fulfillment among us" (1,1).

⁽³³⁾ On Luke's presentation of the conflict Jesus produces among Jews, see D. L. TIEDE, *Prophecy and History in Luke-Acts* (Philadelphia 1980). R. C. TANNEHILL, "Israel in Luke-Acts: A Tragic Story", *JBL* 104 (1985) 69-85, sees Luke's assessment of this division as tragic. In another of his works (*The Narrative Unity of Luke-Acts* [Philadelphia 1986] 172) he cautions against reading Luke's presentation of Pharisees as an historical record of them in the days of Jesus.

⁽³⁴⁾ About Luke's complex presentation of Pharisees, see J. T. CARROLL, "Luke's Portrayal of the Pharisees", *CBQ* 50 (1988) 604-621.

⁽³⁵⁾ DAHL, "The Story of Abraham", 144.

⁽³⁶⁾ Ibid., 148. Dahl cites from Luke the Magnificat, Benedictus, the speech of John the Baptist (3,1-17), the daughter of Abraham (13,16), Zacchaeus (19,9), and from Acts the Stephen speech (7,2-34), Paul's speech in Pisidian Antioch (13,16-41) and Peter's speech (3,12-26).

envisioned Abraham functioning in his theological argument. For Luke the promises to Abraham were but the first in a series that God made to Israel. The prophecies begun in the story of Abraham came to fulfillment in the history of Jesus, and the process of ongoing fulfillment continues in the Christianity of Luke's day. Sometimes he communicated this in less than direct ways by appealing to the figure of Abraham in contexts where parallels or allusions to the patriarch spoke to his audience as effectively as explicit statements (Luke 1,55; 1,72-73; 13,16; 13,28; 16,22-31; 19,9). Even the briefest allusion to the patriarch is capable of indicating the unfolding fulfillment of those promises⁽³⁷⁾. Luke's skill as an author allowed him to use the Jewish and Christian traditions of his day about Abraham to propose an option for apprehending God's plan of salvation, which his readers may have overlooked or were prevented from seeing because of common perceptions about how and to whom salvation comes.

While Dahl is correct about the function of the Abraham story in Luke-Acts, his assessment of Abraham as "a prototype or a model to be imitated" seems too modest. The details of the Zacchaeus story argue in favor of, at least, an exemplary role for Abraham. It is because Zacchaeus has appropriated the example of Abraham that he is a true child of the patriarch, as opposed to those who claim patrimony in name only⁽³⁸⁾. We know that Luke draws this distinction between authentic and inauthentic children of Abraham from John the Baptist's challenge to those who come to him for baptism. He enjoins them to mark their lives by actions and to go beyond merely claiming, "We have Abraham as our father" (3,8).

⁽³⁷⁾ Ibid., 150.

⁽³⁸⁾ Hellenistic Judaism gave Abraham such an exemplary role. For example, in the book of *Jubilees* (20-21) he is presented as a model Jew in a pagan environment. Literature of that sort used Abraham to encourage Jews living in the midst of pagans (cf. SANDMEL, *Philo's Place in Judaism*, 49). According to D. GEORGI, *The Opponents of Paul in Second Corinthians* (Philadelphia 1986) 50: "In proper Hellenistic-Jewish Apologetics the life of Abraham was understood even more emphatically as exemplary for one's own situation. The problem of being Jewish in a pagan environment could be presented by the example of the primeval wanderer. To be sure one saw in him the ancestor of the Jewish people, but at the same time one also knew of his close connection to paganism". This is interesting in relation to Zacchaeus who, as a chief toll collector, would have had contact with pagans. As Abraham was righteous in their midst so also could he be.

That Zacchaeus is a representative of the true line of Abraham is noticed in the way he is portrayed in the story. To show just how much Zacchaeus was an authentic son of Abraham, Luke appealed to what the patriarch was remembered for in Hellenistic-Jewish and Christian tradition: hospitality and justification by works. Connecting Zacchaeus and Abraham that way also allowed Luke to show that the promises made to the patriarch were still in effect. Therefore, he depicted Zacchaeus in the tradition of Abraham's hospitality and cast him as someone who customarily performed acts befitting justification. Zacchaeus received the fulfillment of the promises to Abraham when he extended hospitality to Jesus in Jericho. His customary actions were recognized as righteous by Jesus, who pronounced the salvation for him that Luke understood as a sign of the promises fulfilled. Luke drew on Hellenistic-Jewish and Christian tradition about the patriarch to accomplish this purpose, combining language and allusions from the LXX with other elements of his Jewish and Christian sources⁽³⁹⁾.

He worked mainly with Genesis 18 which provides a number of parallels⁽⁴⁰⁾. Both stories report a meeting at a tree followed by hospitality. There is a correspondence of vocabulary between both episodes: ἀναβλέπειν, ἰδεῖν, κύριος, προτρέχειν, σπεύδειν. Each story concludes with a pronouncement of good news to the host.

One need not expect a verbatim reproduction of the LXX account for Luke to have used it as a source. In fact, his style of using the LXX includes a variety of textual adaptations⁽⁴¹⁾. And so,

⁽³⁹⁾ DAHL, "The Story of Abraham", 140-142, argued that, in his use of the Abraham story, Luke used Jewish and Christian sources available to him. The main Jewish witnesses have been conveniently gathered by SANDMEL, *Philo's Place in Judaism*, 30-211.

⁽⁴⁰⁾ DAHL, "The Story of Abraham", 150, 152-153, shows how Luke imitated biblical historiography, and that he modelled parts of the Magnificat and the Benedictus on references to Abraham in hymnic and historical OT texts.

⁽⁴¹⁾ T. L. Brodie has written extensively on rhetorical *imitatio* in Luke-Acts, although he has not discussed the use of Genesis 18 in Luke 19,1-10. The Zacchaeus story may not qualify as an *imitatio* but some of the ways Luke adapts other LXX stories are relevant. An overview of Brodie's approach is found in "Greco-Roman Imitation of Texts as a Partial Guide to Luke's Use of Sources", *Luke-Acts: New Perspectives from the Society of Biblical Literature Seminar* (ed. C. H. TALBERT) (New York 1984) 17-46. For specific examples see T. L. BRODIE, "Luke 7,36-50 as an Internaliza-

it is not important for Luke to reproduce every detail of the Genesis 18 account, and it is not unusual for him to transform some details. For example, the trees and the action around the trees in each story differ. Also, in the Abraham story he looks up at (ἀναβλέψας) his guests whereas in the Zacchaeus story Jesus looks up at (ἀναβλέψας) his host. Yet there is sufficient similarity between the two accounts to posit Luke's textual dependence on the LXX version of Genesis 18. The following table will show how Luke adapted the Abraham story in Genesis 18 to fit his purpose in telling about Zacchaeus.

Genesis 18	Luke 19
18,1: The Lord appears to Abraham at the oak of Mamre (τῇ ὁρυῖ τῇ Μαμβρη).	19,4: Zacchaeus meets Jesus at a sycamore tree (ἐπὶ συκομορέαν).
18,8: He entertains his guests under a tree (ὕπὸ τὸ δένδρον).	
18,2: Abraham looks up and sees (ἀναβλέψας, εἶδεν) his visitors.	19,5: Jesus looks up at Zacchaeus (ἀναβλέψας).
	19,3: Zacchaeus seeks to see who Jesus is (ἐζήτει ἰδεῖν).
18,2: When he saw them he ran to meet them (ἰδὼν προσέδραμεν).	19,4: Zacchaeus runs to a tree (προδραμών) as Jesus is to pass by (διέρχεσθαι).
18,3: He wishes that they not pass him by (μὴ παρέλθῃς).	
18,7: He runs to the herd (ἔδραμεν).	
18,3: Abraham addresses his visitor as Lord (κύριε).	19,8: Zacchaeus calls Jesus Lord (κύριε).
18,6: Abraham hastens to the tent (ἔσπευσεν).	19,6: Zacchaeus makes haste (σπεύσας), comes down and receives him joyfully (χαίρων).

tion of 2 Kings 4,1-37: A Study in Luke's Use of Rhetorical Imitation", *Bib* 64 (1983) 457-485; id., "Towards an Unravelling of Luke's Use of the Old Testament: Luke 7,11-17 as an *Imitatio* of 1 Kings 17,17-24", *NTS* 32 (1986) 247-267; id., "The Accusing and Stoning of Naboth (1 Kgs 21,8-13) as One Component of the Stephen Text (Acts 6,9-14; 7,58a)", *CBQ* 45 (1983) 417-432; id., "Towards Unraveling the Rhetorical Imitation of Sources in Acts: 2 Kgs 5 as One Component of Acts 8,9-40", *Bib* 67 (1986) 41-67.

In addition to the LXX Luke seems to have relied on other sources of Hellenistic-Jewish tradition about Abraham. These sources support and amplify the biblical tradition about the patriarch in the following ways:

1) Like the LXX they echo the tradition of Abraham's hospitality⁽⁴²⁾.

2) The LXX does not attribute joy to Abraham when he received the angelic messengers in Genesis 18. Philo, however, does mention that a mark of Abraham's hospitality was that "his soul was full of joy" (χαρᾶς) (*De Abr.* 108)⁽⁴³⁾.

3) Abraham is cast as a man of wealth in the Hellenistic-Jewish sources⁽⁴⁴⁾.

4) Furthermore, in them Abraham is known for his justice⁽⁴⁵⁾.

Equally telling for the story of Zacchaeus is something not mentioned about Abraham in the Hellenistic-Jewish or Christian sources. Abraham is never presented as a justified sinner. In his James commentary, M. Dibelius made a strong point about Abraham's justification resulting from the fact that he was a righteous man, "recognized and rewarded by God"⁽⁴⁶⁾.

⁽⁴²⁾ Josephus, *Ant.* 1.11.2 §196; Philo, *De Abr.* 107-114.

⁽⁴³⁾ In Isa 48,22 and 57,21 we find οὐκ ἔστιν χαίρειν τοῖς ἀσεβέσιν, "to rejoice is not for the impious". Luke quotes part of Isa 57,19 in 2,14 and Acts 2,39, so he is familiar with this text. It is not out of the question that he associates joy with Abraham's piety, as does Philo, who knows the LXX tradition that joy is a divine attribute. In *De Abr.* 202, he writes that "rejoicing is most closely associated with God alone". The thought is continued in the following sections of that work as is the idea that Abraham passed on his joy to his descendants. In *Quis rer. div. heres* 7 Philo asks the question in relation to Abraham's discussion with God over an heir, "When, then, is it that the servant speaks frankly to his master?" His answer is that, in addition to when he is sinless and has a clear conscience, it is when "he feels more joy at being a servant of God than if he had been king of all the human race..." (LCL). Luke may have known this Abraham tradition and included Zacchaeus' joy to further illustrate how he is a true child of Abraham.

⁽⁴⁴⁾ Jubilees 17,17; Josephus, *Ant.* 1.8.1 §165; Philo, *De Abr.* 252.

⁽⁴⁵⁾ *Ps. Eupol.* in Eusebius, *Praep. Ev.* 9.17.5; Josephus, *Ant.* 1.10.2 §182; Philo, *De Abr.* 225.

⁽⁴⁶⁾ James (Hermeneia; Philadelphia 1976) 162. Further on (171) he adds: "However, precisely in looking at Philo and even more so in looking at Paul one feature in the Jewish figure must be emphasized; his faith is not

Luke found these traditions helpful in modelling Zacchaeus as an authentic son of Abraham. The similarity of words and themes that show up in Luke's story suggest that they are more than simple allusions and mere coincidence. Indeed they function to connect Zacchaeus with Abraham, showing how genuine a descendant of the patriarch he is. The resultant picture of Zacchaeus as Abraham's faithful heir helps draw a contrast between those who might claim to be children of Abraham and those who rightfully are, and respects the division within Judaism itself that Luke understands Jesus to have caused (2,34; 4,28-30; 9,53)⁽⁴⁷⁾. A very important factor, which distinguishes one group from the other, is their actions. Good deeds testify to the veracity of their claim.

Perhaps the place where Luke articulates that most clearly is in the preaching of John the Baptist (3,1-20). Luke ties "salvation" to good works by linking them in the first fourteen verses of this pericope. The prophetic description of John's mission ends with the eschatological notice *καὶ ὄψεται πᾶσα σὰρξ τὸ σωτήριον τοῦ θεοῦ* (v.6). John's preaching beginning in v.7 is joined to the Isaiah citation by means of *οὖν*, showing how it is fulfilled. This Q material (vv.7-9) is then joined to the section on good works by means of another *οὖν* in v.10. The effect of this arrangement is to challenge those who may have perceived that baptism alone would help them to escape the divine wrath mentioned in v.7. Luke seems to be saying here that "salvation" is a matter of doing; that is what it means to be τέκνον Ἀβραάμ⁽⁴⁸⁾.

the faith of a sinner, a faith by which a sinner comes to God, but rather the faith of the righteous man whose relationship with God is that of close friendship".

⁽⁴⁷⁾ A. GEORGE, "Israël dans l'œuvre de Luc", *RB* 75 (1968) 498.

⁽⁴⁸⁾ It is interesting that Josephus, *Ant.* 18.5.2 §116-17, seemed to understand John's mission in similar terms. He claims that John's baptism was not meant to function apart from good works: "But to some of the Jews the destruction of Herod's army seemed to be a divine vengeance, for his treatment of John, surnamed the Baptist. For Herod had put him to death, though he was a good man and had exhorted the Jews to lead righteous lives, to practice justice towards their fellows and piety towards God, and so doing to join in baptism. In his view this was a necessary preliminary if baptism was to be acceptable to God. They must not employ it to gain pardon for whatever sins they committed, but as a consecration of the body implying that the soul was already thoroughly cleansed by right behaviour..." (LCL).

Those, then, who would call themselves children of Abraham are exhorted by John not to rely on that status in name only, or to seek baptism alone. When they inquire about what further must be done, John tells them to give alms (3,11), to deal justly with others (3,13), and not to rob, defraud, or make false accusations (3,14). The recipients of the preaching and the baptism are made up of the multitudes, toll collectors and soldiers. In 3,21 we are told they were all baptized into John's baptism.

The requirements of salvation in John's preaching are baptism and good works. These do not appeal to all who hear him. We learn, for example, in 7,29-30 that some Jews, Pharisees and lawyers, did not accept the baptism of John. And in the same place we are reminded that all the people and the toll collectors had been baptized by him. Early in the Gospel, then, there is a division among Jews, and John's baptism is partly responsible for it⁽⁴⁹⁾. Luke understood the division caused by John as a precursor to the division that Jesus caused (2,34; 4,28-30; 9,53).

The rejection of Jesus by the murmuring crowd in 19,7 may be a function of this division. In his attitude towards Zacchaeus, Jesus sides with those who had accepted the baptism of John, the ones thought of as outcasts, toll collectors and sinners, and with whom he customarily associates (7,24-34)⁽⁵⁰⁾. The crowd in 19,7, representing

⁽⁴⁹⁾ BRAWLEY, *Luke-Acts and the Jews*, 23. GEORGE, "Israel", 489, traces the division between Jews who are believers and non-believers back to the baptism of John. It is conceivable, then, that Zacchaeus had been included among those who accepted the baptism of John. Since John baptized in all the region of the Jordan, where Jericho is located, Luke may have intended him to be among those Jews who were divided against the Pharisees and lawyers precisely on this point. L. T. JOHNSON, "The Lukan Kingship Parable (Lk. 19:11-27)", *NT* 24 (1982) 156, n.69, noticed how mention of Abraham functions as part of the theme of acceptance and rejection of Jesus by the people in Luke 3,8; 13,15-16; 13,28; 16,22 and 19,1-10.

⁽⁵⁰⁾ TYSON, "The Jewish Public in Luke-Acts", 578, n.18, spells out the terms of the division: on the one side are John the Baptist, Jesus, the people who listen, toll collectors and the children of wisdom. On the other stand the Pharisees, lawyers, the *ἄνθρωποι* of this generation and the children who refused to mourn and dance. It is not necessary to agree with Tyson's conclusion that by the end of Acts there is total rejection of Jesus by Israel in order to see the truth of the division he finds among Jews throughout Luke-Acts. Knowledge of this division heightens the irony in the Parable of the Pharisee and the Toll Collector (18,9-14). The crowd in

the position echoed earlier by the Pharisees and lawyers, is critical of Jesus for this, and so he must come to his own defense by vindicating Zacchaeus as a true son of Abraham⁽⁵¹⁾. It cannot be a mere coincidence, then, that Zacchaeus' defense is made up of precisely the things that John enjoined upon those who received his baptism, almsgiving and justice.

Reading the Zacchaeus story in light of the requirements of salvation according to John's preaching and the division created among Jews as a result of that is instructive for understanding 19,8 as a defense rather than as a resolve. Because Zacchaeus is a true son of Abraham and fulfills the obligations of Abraham's children, as spelled out by John the Baptist, Jesus comes to him and vindicates him with salvation⁽⁵²⁾. In so doing he performs a prophetic act not unlike others in the Gospel which pit him against Pharisees and lawyers, as well as other Jews who do not fall under the status of true children of Abraham. As his statement in 4,21, "Today this scripture is fulfilled in your hearing", functions as a prophetic declaration, so does "Today salvation has come to this house, since he also is a son of Abraham", in 19,9 (*RSV*). It is no accident that the first comes at the beginning of Jesus' ministry and

19,7 seems to embody the attitude of the Pharisee, and those whom the introduction to the parable (18,9) says, "trusted in themselves that they were righteous and despised others". Jesus' action on Zacchaeus' behalf deflates their righteousness, as does his telling the parable.

(⁵¹) TYSON, "The Jewish Public in Luke-Acts", 578, demonstrated how towards the end of the Gospel the crowd progressively adopts the attitude of Jewish leaders and, although it was once supportive of Jesus, it gradually turns against him. To connect the crowd of 19,3 more closely to the portion of Israel that rejects Jesus one need only be reminded that N. A. DAHL, "A People for his Name", *NTS* 4 (1958) 324, has shown ὄχλος to be a synonym for λαός in Luke, which always refers to Israel. Thus in the Zacchaeus story the crowd echoes what the Jewish leaders (Pharisees and lawyers) thought about Jesus earlier in the gospel and joins in the inappropriate judgment they made against him.

(⁵²) The story of the Rich Man and Lazarus (16,19-31) offers yet another contrast showing how another rich person, who claims the patrimony of Abraham but does not give alms to the poor, is excluded from salvation. It too deals with vindication, as does the parable in 18,1-8. In that pericope Jesus asks the question in 18,8, "Nevertheless, when the Son of Man comes will he find faith on earth?" 19,1-10 suggests that the answer is affirmative, for he not only found faith but the good works of Zacchaeus as well.

the second at the end of it, just before he is to enter Jerusalem, the place where prophets die. Both statements frame Jesus' ministry and underline the division among Jews that his preaching caused. But both statements also show the extent to which outcasts, toll collectors, sinners, etc. are capable of receiving salvation.

The question of good deeds is very much at the heart of the chapter immediately preceding the Zacchaeus story. In the parable of the Pharisee and the Toll Collector (18,9-14) presumed righteousness renders the Pharisee's self-perception as presumptuous. The Rich Ruler (18,18-30) cannot follow Jesus because, despite all of his other good actions, he is unwilling to sell his possessions and give to the poor. Both stories echo something of the requirements for true status as children of Abraham. The attitudes of the individuals featured in these two stories contrast with the attitude of Zacchaeus, so by the time the reader gets to 19,1-10 the way has already been prepared for appreciating the value of Zacchaeus' customary actions described in 19,8.

Paying attention to the use of the Abraham traditions in Luke 19,1-10, then, provides one way of resolving some of the ambiguity of whether Zacchaeus' statement in 19,8 is one of resolve or defense. If Zacchaeus is a representative of the faith and works of his ancestor Abraham, then he portrays a righteousness that Luke uses to account for his visit from Jesus and its effects. Interpreting 19,8 as a defense shows that Zacchaeus did not merely claim the patrimony of Abraham but made good on that claim by his deeds.

Zacchaeus' customary actions, expressed in 19,8, demonstrated that he was a true son of Abraham, for his claim to give alms and to make restitution authenticated his status as a Jew. To that extent he is not unlike other exemplary Jews portrayed in the Gospel and Acts (e.g. Elizabeth, Zechariah, Mary, Joseph, Anna, Simeon, Gamaliel), those who represent an openness to Jesus and Christianity that enables them to receive the promises made to Abraham. He stands opposite to Jews who reject Jesus and criticize him openly, as well as to those who claim the patrimony of Abraham but do not honor it (3,8; 16,19-31). He is worthy of inclusion in the "family" of Jesus because he is one who hears the word of God and does it (8,21; 11,28).

Conclusion

The four usual arguments for reading Luke 19,8 as Zacchaeus' resolve, marking his repentance, have not settled the question of how to understand the present tenses of δίδωμι and ἀποδίδωμι. A close examination of them suggests that Luke 19,8 makes better sense, read in the context of Luke-Acts, as a statement of Zacchaeus' defense of his customary actions. The present tenses of δίδωμι and ἀποδίδωμι will bear the iterative sense, and the immediate and wider contexts of this story argue in favor of that interpretation. My analysis of Luke's use of Hellenistic-Jewish and Christian traditions about Abraham supports the understanding of 19,8 as a defense of Zacchaeus' customary actions, which mark him as a true "son of Abraham". Therefore, the interpretation of Luke 19,8 presented here confirms the minority opinion of scholars who interpret it as a defense and adds further reasons for understanding it so.

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SOMMAIRE

Les quatre arguments grâce auxquels on lit Lc 19,8 comme une résolution de Zachée marquant la repentance n'ont pas encore satisfait à la question suivante: comment comprendre les présents *didōmi* et *apodidōmi*. Un examen attentif de ces verbes suggère que Lc 19,8 s'explique mieux si on le lit dans le contexte de Lc-Ac, comme une déclaration de défense de Zachée relative à sa pratique habituelle. Les présents *didōmi* et *apodidōmi* auront un sens itératif, et les contextes proche et lointain de ce récit appuient cette interprétation. Une analyse de l'usage lucanien des traditions juive et chrétienne au sujet d'Abraham favorise la compréhension de 19,8 comme pratique habituelle, qui fait de lui un vrai « fils d'Abraham ». En conséquence, l'interprétation de Lc 19,8 présentée ici confirme l'opinion minoritaire des exégètes qui l'interprètent comme une défense et apporte de nouvelles raisons pour la comprendre ainsi.

John 5,2 and the Date of the Fourth Gospel⁽¹⁾

John is aging rapidly. In the past one hundred and forty years or so, biblical scholars have added over two hundred candles to the birthday cake of the fourth gospel. In the mid-nineteenth century, that Hegelian-minded professor at Tübingen, F.C. Baur, dated John as late as AD 170⁽²⁾. But even in this century a few die-hards have clung to an unbelievably late date. For example, in 1925 Delafosse saw 170-75 as the ceiling⁽³⁾ and in 1936 Loisy felt that "the first publication can hardly have been effected before 135-40"⁽⁴⁾. He opted for 150-60 as a more viable date.

But with the discovery of a tiny scrap of papyrus some fifty years ago⁽⁵⁾, such a radical view of the date of this gospel has bitten the proverbial dust. John Rylands Papyrus 457 — better known as P⁵² — includes portions of five verses from John 18 and is dated almost unanimously within the first half of the second century⁽⁶⁾. Add to this the conceptual and verbal parallels between John and

(1) This is a revision of a paper originally read to the Fourth Gospel Group at the Society of Biblical Literature Meeting held in Boston, MA, on December 7, 1987. Thanks are due to professors W. Hall Harris, Harold W. Hoehner and H. Simian-Yofre for reading a preliminary draft of this paper and making many valuable suggestions.

(2) See his lengthy treatment on this and other issues in "Ueber die Komposition und den Charakter des Johannesevangeliums", *Tübinger Theologisches Jahrbuch* 3 (1844) 1-191, 397-475, 615-700.

(3) H. DELAFOSSE, *Le Quatrième Évangile* (Paris 1925) 127.

(4) A. LOISY, *The Origins of the New Testament*, trans. L. P. Jacks (London 1950; French ed. 1936) 193.

(5) C.H. ROBERTS, *An Unpublished Fragment of the Fourth Gospel in the John Rylands Library* (Manchester 1935).

(6) The general consensus is that P⁵² should be dated about 125 (K. ALAND, *Studien zur Überlieferung des neuen Testaments und seines Textes* [Berlin 1967] 91). If so, John can hardly have been written later than 100, especially since Papyrus Egerton 2, written at about the same time as P⁵², draws on both the synoptic gospels and John (see C.H. DODD, *BJRL* 20 [1936] 56-92). But the fact that some biblical scholars still affirm (or at least allow for) a date (of John) some years after 100 is less excusable (e.g., C. K.

Qumran⁽⁷⁾ as well as the excavations of the last sixty or so years in Palestine which dramatically confirm John's topographical accuracy⁽⁸⁾, and one has a threefold cord of evidence to substantiate that this gospel is a first-century document.

BARRETT, *The Gospel According to St. John* [London 1955], suggests "the wide limits of A.D. 90-140" [108]; M.S. ENSLIN, *The Literature of the Christian Movement* [New York 1956], argues that "it would seem most likely to have been penned during the first three or four decades of the [second] century" [451]; J.C. FENTON, *The Gospel According to John in the Revised Standard Version* [Oxford 1970], thinks that "perhaps A.D. 140 is the latest possible date of writing" [16]; R. BULTMANN, *The Gospel of John: A Commentary* [Philadelphia 1971], sees 120 as the upper limit [12]; D.J. SELBY, *Introduction to the New Testament* [New York 1971], holds to "a date not later than the early decades of the second century" [79]).

On the other hand, there are a few scholars who suggest a date earlier than 125 for P⁵² which would effectively push the date of the gospel well within the first century. E.g., A. DEISSMANN, "Ein Evangelienblatt aus den Tagen Hadriens", *Allgemeine Evangelisch-Lutherische Kirchenzeitung* 69 (January 3, 1936) 19-20, considered it to have been written in the early years of Hadrian's reign (117-138) or even during Trajan's (98-117). With this date W.F. ALBRIGHT concurred (*The Archaeology of Palestine* [Baltimore 1960] 239). J. MARSH, *The Gospel of St John* (Baltimore 1968), states that the date for this papyrus "may well be several decades earlier" than 150 (27), a point already acknowledged by Roberts. K. ALAND, "Neue neutestamentliche Papyri II", *NTS* 9 (1962-63) assigns a date near the beginning of the second century (307). B.M. METZGER, *Manuscripts of the Greek Bible: An Introduction to Greek Palaeography* (Oxford 1981), allows 100 to be the lower limit (62). H. STAUDINGER, *The Trustworthiness of the Gospels* (Edinburgh 1981), thinks that P⁵² "may well be given a date at the end of the first or the beginning of the second century" (15). Finally, H.H. HOBBS, *An Exposition of the Gospel of John* (Grand Rapids 1968), suggests the possibility, citing [W.] Schubart as his authority, that P⁵² was penned during the reign of Domitian (10)! Unfortunately, Hobbs does not document this statement by Schubart. To be sure, only a handful of scholars date P⁵² this early, and of course all are tentative, for it is difficult to pinpoint the date of a MS within a period smaller than 50 years (METZGER, *Manuscripts*, 50).

(⁷) See D. CROSSAN, *The Gospel of Eternal Life: Reflections on the Theology of St. John* (Milwaukee 1967) 4, n.4, for a brief but decent bibliography on the relation of Qumran to John. As well, cf. the specific study by J.H. CHARLESWORTH, "A Critical Comparison of the Dualism in 1QS III, 13 — IV, 26 and the "Dualism" contained in the Fourth Gospel", *NTS* 15 (1968-69) 398-418.

(⁸) See J.A.T. ROBINSON, *Redating the New Testament* (Philadelphia 1976) 278, n. 122, for a succinct yet helpful bibliography on John's accuracy about the topography of Palestine before AD 70.

Indeed one of the facts about the remarkable scholarly consensus... on the dating of the Johannine literature is that it cuts across almost every possible division. Those who believe that all five books — the Revelation, the gospel and the three epistles — are by one man, and that man the apostle John, and those who hold to none of these, or to almost every possible permutation of them, find common ground in dating both the Revelation and the gospel and epistles in the years $\pm 90-100$ (°).

But upon the publication of J. A. T. Robinson's *Redating the New Testament* in 1976, in which he argues for a pre-70 date for *all* the New Testament books, it is becoming more fashionable to affirm a date for John's gospel before the fall of Jerusalem — especially since such a view has gained respect from a non-conservative scholar the stature of Robinson⁽¹⁰⁾. In other words, there is a small but growing minority who would like to add another thirty or so candles to the birthday cake of the fourth gospel.

I, too, am intrigued by the possibility that John was published before the razing of Jerusalem. However, I wish to throw the spotlight on an internal argument which has received very little air-time from either friend or foe. I refer to an inference made from the present tense in John 5,2 — “Now there is in Jerusalem, by the sheep[gate], a pool (the one called Bethesda in Hebrew) which has five porticoes”⁽¹¹⁾. It is my purpose in this paper to show that this

(°) *Ibid.*, 254.

(10) ROBINSON, *Redating*, 307-308, n.218, gives an impressive list of scholars who argue for a pre-70 date of John. To this list we might add the names of Bengel, though Robinson does mention Bengel elsewhere (278, n.123), Lampe, Lardner, Ryle, Wegschneider and Wettstein to his ‘forgotten names’ list. More recently, we can add F. BLASS, “The Origin and Character of our Gospels. III: St. John”, *ExpTim* 18 (1906-07) 458, which, incidentally, is a surprising omission by Robinson; MARSH, *The Gospel of St John*, 25-30; F. L. CRIBBS, “A Reassessment of the Date of Origin and the Destination of the Gospel of John”, *JBL* 89 (1970) 55, though Robinson does mention him in another place (268, n.79); and J. FINEGAN, *The Archeology of the New Testament: The Mediterranean World of the Early Christian Apostles* (Boulder 1981) 44-46. Perhaps even E. E. ELLIS, “Dating the New Testament”, *NTS* 26 (1979-80) 488-492, 500-502, belongs here.

(11) J. JEREMIAS, *The Rediscovery of Bethesda: John 5:2* (Louisville 1966), argues for (1) ‘sheep-pool’ instead of ‘sheep-gate’, and (2) ‘Bethesda’ instead of ‘Bethsaida’, etc., in the translation of this text (9-12). Against this, see D. J. WIEAND, “John V.2 and the Pool of Bethesda”, *NTS* 12 (1965-66) 392-394.

verse may well make an important contribution to the question of the date of the fourth gospel.

I. The *Insignificant* Role of John 5,2 in Dating the Fourth Gospel

One of the pillars upon which Bengel built his argument that John was published before AD 70 was the natural inference from John 5,2 that Jerusalem was still completely intact⁽¹²⁾. One and a half centuries later a pronouncement by the erudite German scholar Theodor Zahn, virtually laid this piece of evidence to rest: "The attempt has been made frequently, but manifestly without warrant, to prove from the present tense in v.2 that the Gospel of John was written before the year 60 [*sic*]..."⁽¹³⁾.

The result has been that the twentieth-century debate over the date of the fourth gospel rarely considers John 5,2. Schnackenburg, for example, in his massive three-volume commentary on John, relegates his entire discussion of the present tense, ἔστιν, used in this text to a one-sentence footnote: "It is hardly possible to conclude from the use of the present tense ἔστιν that the structure was still in existence at the time that the story was written since it may be a present historic"⁽¹⁴⁾. Bultmann does the same, summarily dismissing the evidence as highly improbable for a pre-70 date⁽¹⁵⁾. Bruce, however, gives the issue a bit more dignity by rejecting the natural force of ἔστιν in the text of his commentary⁽¹⁶⁾. The rather full commentaries by Barrett⁽¹⁷⁾, Brown⁽¹⁸⁾ and Lindars⁽¹⁹⁾, how-

(12) J. A. BENDEL, *Gnomon Novi Testamenti* (Tübingen 1759): "Scriptis Ioannes ante vastationem urbis" (358).

(13) T. ZAHN, *Introduction to the New Testament* (Edinburgh 1909) 3:335. To be sure, the death knell was sounded not merely because of this one-liner but also because of a detailed refutation which followed (349, n. 6).

(14) R. SCHNACKENBURG, *The Gospel According to St John* (New York 1982) 2:460, n. 9.

(15) "ἔστιν v.2 hardly proves that at the time of the narrator the temple had not yet been destroyed (Schl.); nor does ἦν 11.18 prove the contrary" (BULTMANN, *The Gospel of John*, 240, n. 4).

(16) F. F. BRUCE, *The Gospel of John* (Grand Rapids 1983) 121. Nevertheless, his comment, though part of the text, is in parentheses!

(17) BARRETT, *The Gospel According to St John*.

(18) R. E. BROWN, *The Gospel According to John*, 2 vols. (Garden City, NY 1966).

(19) B. LINDARS, *The Gospel of John* (Grand Rapids 1972).

ever, ignore the ἔστιν altogether⁽²⁰⁾. So final was Zahn's pronouncement on this matter, in fact, that I was unable to find any modern author who held to a post-70 date for John to put more than two or three sentences together in a refutation of the implications of this verse⁽²¹⁾.

Remarkably, in the opposing camp the discussions are equally thin. Most notably, in an article by F.L. Cribbs⁽²²⁾, which proposed an early date for John based on internal considerations exclusively⁽²³⁾, no discussion of 5,2 was found. The same curious omission is true of Goodenough's provocative study⁽²⁴⁾, as well as Turner and Mantey's commentary⁽²⁵⁾; even Turner's later article on the date of John neglected this possible internal clue⁽²⁶⁾.

Nevertheless, the pre-70 camp has not been completely demoralized. Leon Morris⁽²⁷⁾ and J.A.T. Robinson⁽²⁸⁾ have stood

⁽²⁰⁾ Brown in fact goes so far as to declare that he finds "nothing in John that would demand a date before 70 for the final written form of the Gospel" (*The Gospel According to John*, 1: LXXIV).

⁽²¹⁾ The most complete refutations I have seen, apart from Zahn's, were those by J. MOFFATT, *An Introduction to the Literature of the New Testament* (Edinburgh 31918) 581-582, who basically says the same thing as Zahn, though in much abbreviated form; and JEREMIAS, *Rediscovery*, 14 and n. 21.

⁽²²⁾ "Reassessment", 38-55.

⁽²³⁾ *Ibid.*, 39.

⁽²⁴⁾ E. R. GOODENOUGH, "John A Primitive Gospel", *JBL* 64 (1945) 145-182.

⁽²⁵⁾ G. A. TURNER - J. R. MANTEY, *The Gospel According to John* (Grand Rapids 1958).

⁽²⁶⁾ G. A. TURNER, "The Date and Purpose of the Gospel by John", *BETS* 6 (1963) 82-85.

⁽²⁷⁾ L. MORRIS, *Studies in the Fourth Gospel* (Grand Rapids 1969) 289 and *The Gospel According to John* (NICNT; Grand Rapids 1971) 33, 299-300. However, in his most recent (popular) treatment he attaches no weight to this text in support of an early date (*The Word was Made Flesh, John 1-5* [vol. 1 of *Reflections on the Gospel of John*] [Grand Rapids 1986] 165).

⁽²⁸⁾ *Redating*, 277-278. Apart from Zahn's refutation (and Blass's defense — see n. 10), Robinson has the most complete discussion on this point that I have seen. Yet even in the midst of his argument he adds, "Too much weight must not be put on this..." (278). M. de Jonge, in his critique of Robinson's work, concurs: "His attempt to build a better case on the present ἔστιν in v. 2 is hardly convincing" (M. DE JONGE, "The Beloved Disciple and the date of the Gospel of John", *Text and Interpretation: Studies in the New Testament presented to Matthew Black* [ed. E. BEST - R. McL. WILSON] [Cambridge 1979] 114, n. 4). In Robinson's final contribution to

apart from modern scholars⁽²⁹⁾ in arguing that John 5,2 perhaps makes some contribution, even though secondary, to the date of the fourth gospel⁽³⁰⁾.

It is my contention, however, that the arguments against the natural force of ἔστιν in John 5,2 are by no means airtight and that in fact this particular piece of internal evidence may be of great value in determining the date of this gospel⁽³¹⁾.

II. The Bearing of John 5,2 on the Date of the Fourth Gospel

If one wishes to circumvent the *prima facie* implications of John 5,2, and hence an early date for John's gospel on this basis, there are five routes open to him⁽³²⁾. First, he might view this verse as the vivid recollections of an old man in which John uses the present

the date of John, he sees no significance in the present tense: "Indeed the presumption is that it is still standing as the evangelist himself describes it in the three present tenses of 5.2 (though nothing can be built upon them) ..." (J. A. T. ROBINSON, *The Priority of John* [London 1985] 70).

⁽²⁹⁾ C. C. Tarelli's sidenote about John 5,2 probably best captures the modern sentiment by advocates of the pre-70 view: "To suggest a date before A.D. 70 is perhaps too daring, and yet the Palestinian atmosphere which many scholars find in the Gospel is certainly the atmosphere of Palestine before that date, however little weight is to be attached to v. 2: 'There *is* in Jerusalem'" (C. C. TARELLI, "Clement of Rome and the Fourth Gospel", *JTS* 48 [1947] 209).

⁽³⁰⁾ I have found only two other twentieth-century authors who lean on John 5,2 in support of a pre-70 date of John. The first, O. B. GREENE, *The Gospel According to John* (Greenville, SC 1966) 263, can hardly be said to have written a major work. The other, F. Blass, has a posthumous series of essays on "The Origin and Character of our Gospels", *ExpTim* 18 (1906-07) (see n. 10).

⁽³¹⁾ It should be pointed out, however, that even if John 5,2 is given its full force, this would not necessarily demand a pre-70 date for the entire gospel. If the gospel went through various stages of production/publication, this verse might only affirm an early date for 5,1-47 (assuming that the extended discourse on the Sabbath is an integral part of the healing story). For a discussion of this possibility, see section II. 4.

⁽³²⁾ There is also, marginally, a sixth approach, viz., to regard ἦν as the original reading here and ἔστιν as a scribal corruption. I have seen but one author to take this route (G. CAMPBELL, *The Four Gospels, Translated from the Greek, with Preliminary Dissertations*, vol. 2: *Mark-John* [Andover 1837] 490).

tense to describe a place which has long since disappeared. Technically, this is known as a historical present. Secondly, John may be using ἔστιν in some other, anomalous manner in which he legitimately speaks of a now dead spot on the map as though still alive. Thirdly, John may have erred: if the tradition of Ephesus (or, more generally, Asia Minor) as the place of publication is accurate, then the author might not have known that the pool and/or its porticoes had been devastated when Titus leveled the city. Fourthly, the pool of Bethesda might have survived the Jewish War, remaining intact by the time John's gospel was penned. Finally, the gospel might have been composed in stages with some material written before 70 (including 5,2), though the final form would not have been published until the last decade of the first century.

On the one hand, any one of these views, if true, would negate the force of John 5,2 for an early date of the gospel. On the other hand, if it could be demonstrated that each one of them is false, such a demonstration would reveal, on this one strand of evidence, the inherent improbability that John's gospel was published after the destruction of the holy city. A critique of each view is therefore in order. We will deal with these five views according to what most would perceive to be an ascending order of formidability.

1. *The Error View*

The weakest approach is the one which assumes that the author made a mistake when he wrote that the pool was still intact after 70. In order to commit such a blunder, John would have to be completely ignorant of the city's devastation twenty or thirty years after the event. We hardly need to dispute such an argument, for this view is irrational on its face: it is virtually impossible for any Jew or Christian of the late first century — regardless of his domicile — not to know of the results of the war between Jerusalem and Rome⁽³³⁾. And it would be singularly absurd to suggest that the writer of the only gospel to emphasize Judea over Galilee would not keep abreast of news concerning Jerusalem. One should not be

⁽³³⁾ Regardless of what else one thinks of his book, Robinson does make a good case for 'the significance of 70' to both Jew and Christian alike (*Redating*, 13-30). For Christians this would be an apologetic weapon which would, in all likelihood, become widely known within months of the final siege of the city.

surprised, therefore, to learn that no writer in this century (as far as I am aware) even entertains this hypothesis⁽³⁴⁾.

2. *The Anomalous Present View*

A stronger case could be made for the view that John uses the present tense in some unusual way, in which he refers to a structure now in ruins. Apart from the historical present (an approach we will look at later) the only other candidate is the customary present.

The argument is as follows: the epistle to the Hebrews was written after 70, yet it uses the present tense to refer to the Jewish sacrificial system which had already ceased. Thus, ἔστιν in John 5,2 might also belong to this category, therefore indicating nothing about the time of writing.

I have not seen this analogy explicitly made by Johannine scholars, though McNeile, in discussing the date of Hebrews, states:

...it is recognized by most modern writers on the epistle that the references in the present tense to Old Testament worship... afford no evidence that the Temple was still standing. See John 5:2 'There is in Jerusalem at the sheepgate a pool' ⁽³⁵⁾.

Moffatt turns this around and tries to argue that the present tenses in Hebrews nullify the natural force of ἔστιν in John 5,2 ⁽³⁶⁾. On the one hand, such arguments are quite circular (i.e., the assumed late date of John is pressed into service for Hebrews and vice versa). On the other hand, we cannot argue against the fact of present tenses being used for something that took place in the past (at least as Moffatt has presented it) for even if we date Hebrews *before* 70, such present tenses are still used by Josephus and patristic writers in referring to the defunct levitical system ⁽³⁷⁾. The basic problem with this approach, however, is that it suffers from semantic myopia.

⁽³⁴⁾ F. BLEEK, *An Introduction to the New Testament*, 2 vols. (Edinburgh 1869) is open to the 'error' view without committing himself (317-318), while A. HOVEY, *An American Commentary on the New Testament*, vol. 3: *Commentary on the Gospel of John* (Philadelphia 1885) perhaps is alluding to others who embraced this view (129).

⁽³⁵⁾ H. MCNEILE, *An Introduction to the Study of the New Testament* (2d. ed. rev. C. S. C. Williams) (Oxford 1953) 236.

⁽³⁶⁾ *Introduction*, 582.

⁽³⁷⁾ Cf. Josephus, *Jewish Antiquities* 3.224-257; *Against Apion* 2.77, 193-198; *1 Clem.* 41:2; *Barn.* 7-8; *Diogn.* 3.

That is, it compares the customary present in Hebrews with a copula in John. The two categories simply do not mix. A customary present is, to put it colloquially, an iterative present with the temporal ends 'kicked out' ⁽³⁸⁾. It is repeated action over a long period of time. For example, in Heb 9,25 we read that "the high priest *enters* [εἰσέρχεται] the Holy of Holies annually". In 10,1 we are told that "year after year they (repeatedly) *offer* [προσφέρουσιν] the same sacrifices" ⁽³⁹⁾. But a place — such as a pool — does not exist over and over again. In short, the very nuance of the customary present excludes the equative verb from its ranks. One cannot legitimately appeal to the present tense in Hebrews, then, as affording a legitimate parallel to ἔστιν in John 5,2.

3. *The Intact View*

A third approach is one which argues that the pool survived the Jewish War intact. This view — in my mind anyway — is the most formidable opponent to the one proposed in this paper, though it has gained surprisingly few adherents (hence it stands third in the list).

A century ago, Alfred Plummer argued that "even if the building [i.e., the porticoes] were destroyed the pool would remain; and such a building, being of the nature of a hospital, would possibly be spared" ⁽⁴⁰⁾. A few others have followed in his train,

⁽³⁸⁾ Cf. H.E. DANA and J.R. MANTEY, *A Manual Grammar of the Greek New Testament* (Toronto 1927) 183; R.W. FUNK, *A Beginning-Intermediate Grammar of Hellenistic Greek* (Missoula 1973) 2:615 (§782.3). Many grammars, in fact, lump the customary present in with the iterative (cf. A.T. ROBERTSON, *A Grammar of the Greek New Testament in the Light of Historical Research* [Nashville 1934] 880; W.W. GOODWIN, *Greek Grammar* [rev. C.B. Gulick] [Boston 1930] 268 [§235.a]; H.W. SMYTH, *Greek Grammar* [rev. G. Messing] [Cambridge, MA 1956] 421 [§1876]).

⁽³⁹⁾ Cf. also 9,6 (εἰσίσαιιν), 9,9 (προσφέρονται), 9,13 (ἀγιάζει), 13,11 (κατακαίεται), etc., for the kind of present tense which occupies this discussion. But some might challenge the customary present label on these verbs in Hebrews, preferring to call them historical presents (in particular, J. AUSTING, "The Use of the Present Tense for a Past Dispensation — A Problem in Translation", *Notes on Translation* 65 [1977] 28-36). However, such verbs do not easily fit the semantic situation of the historical present (see later discussion in II. 5).

⁽⁴⁰⁾ A. PLUMMER, *The Gospel According to St John* (Cambridge 1882) 131.

though they generally consider this a last-ditch approach, favoring the historical present hypothesis to this one⁽⁴¹⁾.

A fundamental problem with this view is that it attempts to divorce the porticoes from the pool. That is, the view requires only that the *pool* remain intact, but allows the porticoes to be in ruins⁽⁴²⁾. But John will not let us do this, for he writes: "there is... a pool... which *has* five porticoes" (ἔστιν... κολυμβήθρα... πέντε στοάς ἔχουσα). The present participle ἔχουσα is contemporaneous with the main verb ἔστιν⁽⁴³⁾; thus when John wrote this note, both pool and porticoes would have been standing. To argue, then, that the pool survived the catastrophe of 70 is insufficient in itself; if this view is to be viable, the pool and all five porticoes⁽⁴⁴⁾ must be in place at the time the gospel was written.

(⁴¹) Even Plummer preferred the historical present view (*ibid.*). See also (*ad loc.*) the commentaries by A. CLARKE, *The Holy Bible, containing the Old and New Testament* (rev. T. Smith) (New York n.d.); M. DODS, *The Gospel of St. John in the Expositor's Greek Testament, vol. 1: The Four Gospels* (ed. W. R. NICOLL) (reprinted Grand Rapids 1961); F. GODET, *Commentary on the Gospel of John* (New York 1886); O. B. GREENE, *The Gospel According to John* (Greenville, SC 1966); R. C. H. LENSKI, *The Interpretation of St. John's Gospel* (Columbus, OH 1942); A. THOLUCK, *A Commentary on the Gospel of St John* (Edinburgh 1866); D. THOMAS, *The Genius of the Fourth Gospel* (London 1885) 1:120; G. H. TRENCH, *A Study of St John's Gospel* (London 1918); and B. WEISS, *Das Johannes-Evangelium* (MeyerK; Göttingen ⁹1909).

(⁴²) Clarke is one of two exceptions: "This is thought by some to be a proof that... the pool *and its porticoes* were still remaining [*italics added*]" (*The Holy Bible*, vol. 5, n.p. [*ad loc.*]). Cf. also E. W. HENGSTENBERG, *Commentary on the Gospel of St John*, 2 vols. (Edinburgh 1865) 1:256 (see n. 43).

(⁴³) Among the commentators I had occasion to look at, Hengstenberg alone explicitly perceived this point: "The ἔστι need not be explained by supposing that the pool still remained after the destruction of Jerusalem. For it is not the pool in itself which is regarded, but the pool in its property as a sanitary institution, *with its five porches* [*italics added*]" (HENGSTENBERG, *Gospel of St John*, 1:256). Grammarians, however, know it well: cf. E. D. BURTON, *Syntax of the Moods and Tenses in New Testament Greek* (Chicago ³1900) 54 (§119); J. H. MOULTON, *A Grammar of New Testament Greek*, 4 vols. (Edinburgh 1908-76), vol. 3: *Syntax*, by N. TURNER (79); L. RADERMACHER, *Neutestamentliche Grammatik* (Tübingen ²1925) 155; C. F. D. MOULE, *An Idiom-Book of New Testament Greek* (Cambridge ²1959) 99; B. L. GILDERSLEEVE, *Syntax of Classical Greek from Homer to Demosthenes*, 2 vols. (New York 1900-1911) 1:138-139 (§330-331).

(⁴⁴) John's explicit mention of πέντε στοάς as the direct object of ἔχουσα makes this necessary.

A second difficulty for the 'intact' approach — for the older commentators at least — is that it was merely a guess, usually based on the supposition that the Romans would have preserved the pool upon seeing its obvious medicinal value⁽⁴⁵⁾. This argument leaves much to be desired, for the medicinal value of the pool is clearly linked with the supernatural activity of Israel's God⁽⁴⁶⁾ — at best an irrelevant consideration for the invading Roman troops. Should one really pin his hopes of a late date for John — on this piece of evidence at least — on the supposed kindness of the Roman soldiers? Without corroborative evidence, this argument must remain a long shot for plausibility.

Fortunately, corroborative evidence *has* been produced. In 1949 Joachim Jeremias published a thin monograph entitled *Die Wiederentdeckung von Bethesda. Johannes 5,2* which was revised and translated in 1966. *The Rediscovery of Bethesda: John 5:2* traces the architectural history of the pool(s) of Bethesda from the time of Jesus to the modern day. Though it is not his main objective to disprove an early date for John on the basis of this verse, Jeremias' work has become the principal source-book for the 'intact' view⁽⁴⁷⁾.

⁽⁴⁵⁾ So Plummer, Dods, Weiss.

⁽⁴⁶⁾ This is evident even if one considers v.4 to be a later addition, because the crippled man's response to Jesus' question in v.7 implies that the pool's healing powers were both instantaneous and discriminatory. Furthermore, the strong external testimony for v.4 suggests a relatively early and widespread tradition — even if not a part of the autograph. (For a defense of the authenticity of this verse, see Z. C. HODGES, "The Angel at Bethesda — John 5:4", *BSac* 136 [1979] 25-39; against this, see G. D. FEE, "On the Inauthenticity of John 5:3b-4", *EvQ* 54 [1982] 207-218.)

⁽⁴⁷⁾ JEREMIAS, *Rediscovery*, 30, notes that the pool was in reality a twin pool which formed a huge trapezoid, having "a water surface of over 5,000 square meters".

We are assuming, along with the vast majority of scholars, that Jeremias' identification of the twin pool with the pool mentioned in John 5:1-9 is correct. G. BÁEZ-CAMARGO, *Archaeological Commentary on the Bible* (Garden City, NY 1986) 228, sums up current scholarly opinion aptly: "The traditional identification is practically certain, although there has been some discussion about it"; differently, cf. P. BENOIT, "Découvertes archéologiques autour de la piscine de Bethesda", *Jerusalem Through the Ages: The Twenty-fifth Archaeological Convention* (ed. J. AVIRAM) (Jerusalem 1968) 48-57; A. DUPREZ, *Jésus et les Dieux Guérisseurs* (Paris 1970) 28-127; J. KLINGER, "Bethesda and the Universality of the Logos", *St. Vladimir's Theological Quarterly* 27 (1983) 171-178. The viewpoint represented by Benoit, Duprez and Klinger is answered in Jeremias (34) and Wieand (396-397).

In a lengthy footnote Jeremias addresses the issue of the porticoes: "The sources contradict themselves in their statements about the porticoes. A portion of the early Christian writers report that they no longer existed Other sources state that the porticoes still stood" (48). He then cites another authority approvingly: "With reference to the fact that most of the capitals and column fragments have been found in the upper rubbish strata of the pools, van der Vliet ... has illuminatingly explained the contradictory statements: in the 4th-6th centuries the porticoes had indeed collapsed, but for the most part, the columns still stood" (49).

If true, then the 'intact' view could possibly fit the statement in John 5,2. But in order to harmonize these apparent discrepancies among the Christian writers, all the authors who claimed that the porticoes were in ruins would have to have written after the last testimony that they still stood. Unfortunately, the evidence does not fit such a neat package. The first authority to mention the pool since the fall of Jerusalem is the historian Eusebius. Before AD 324 he wrote in his *Onomasticon* of the pool as "*formerly* [having] five porticoes" (50). The next witness is the Pilgrim of Bordeaux who, in 333, spoke of "a pool which *has* five porticoes" (51). One of these writers has to be wrong. If the 'false' witness is the Bordeaux Pilgrim, then the Jeremias-van der Vliet hypothesis cannot

(48) JEREMIAS, *Rediscovery*, 18, n. 38.

(49) Ibid. N. VAN DER VLIET, "*Sainte Marie où elle est née*" et la *Piscine Probaticque* (Paris 1938) 144, however, introduces his hypothesis with some reservations: "Il est donc assez malaisé de conclure, après examen des textes, pour ou contre l'existence des portiques".

(50) Italics added. The full text reads as follows: Βηζαθά. κολυμβήθρα ἐν Ἱερουσαλὴμ. ἥτις ἐστὶν ἡ προβατική. τὸ παλαιὸν ἐ' στοὰς ἔχουσα (cited in D. BALDI, *Enchiridion Locorum Sanctorum. Documenta S. Evangelii Loca Respicientia* [Jerusalem 1955] 456. For a more accessible source, cf. C. TISCHENDORF, *Novum Testamentum Graece* [Lipsiae 1869] 1:783 [*ad loc.* John 5,2]). The adverbial use of τὸ παλαιὸν here seems to imply the distant past — an implication clearly against the van der Vliet-Jeremias hypothesis, though fitting nicely with the view proposed in this paper (see LSJ 1290 [s.v. παλαιός] who distinguish τὸ παλαιὸν meaning 'formerly' from ἐκ παλαιού meaning 'from of old').

(51) Italics added. The original Latin reads: *interius uero ciuitati sunt piscinae gemellares, quinque porticos habentes, quae appellantur Betsaida. Ibi aegri multorum annorum sanabantur* (cited in P. GEYER, *Itinera Hierosolymitana Saeculi IIII-VIII* [CSEL 39; Prague 1898] 21; BALDI, *Enchiridion*, 456; TISCHENDORF, *Novum Testamentum Graece, ad loc.* [John 5,2]).

stand⁽⁵²⁾. In fact, Jeremias mentions only one other authority to side with the Pilgrim — an anonymous writer from the sixth century⁽⁵³⁾. On the other side, however, besides Eusebius, Cyril of Jerusalem (before AD 386)⁽⁵⁴⁾, Jerome (before 420)⁽⁵⁵⁾, Theodore of Mopsuestia (by 428)⁽⁵⁶⁾ and Pseudo-Athanasius (sixth century)⁽⁵⁷⁾ all speak of the porticoes in the past tense. Thus, in light of the fact that for our view the authorities are weightier, more numerous and earlier, it seems most prudent to abandon the 'intact' view, at least in its present form, altogether⁽⁵⁸⁾.

⁽⁵²⁾ Significantly, van der Vliet dated Eusebius' comments at AD 340. But this was the date of his death, not the date of *Onomasticon*. By this mental slip, van der Vliet was then able to conclude that the Pilgrim of Bordeaux's testimony preceded that of Eusebius by seven years. However, Jeremias, who corrected the date of Eusebius' testimony, did not seem to notice that van der Vliet's hypothesis was thereby undermined!

⁽⁵³⁾ The Anonymous Pilgrim of Piacenza (c. 570) in *Itinerarium*, 27, spoke of "the swimming pool which has five porticoes... in which many miracles are performed" (*piscina natatoria, quae habet quinque porticus... in qua multae fiunt uirtutes*) (GEYER, *Itinera*, 177; BALDI, *Enchiridion*, 457).

⁽⁵⁴⁾ Ἐν γὰρ τοῖς Ἱεροσολύμοις ἦν προβατικὴ κολυμβήθρα, πέντε στοὰς ἔχουσα (*Homily on the Lame Man at the Pool* 2; PG 33, col. 1133; BALDI, *Enchiridion*, 456): "For in Jerusalem, there was a sheep pool, which had five porticoes". Perhaps Cyril's testimony ought to be discounted because he speaks of the pool as well as the porticoes with the imperfect tense (ἦν). This, of course, does not imply that the pool had ceased to exist, nor does it imply that the porticoes continued to stand (see our discussion of the historical present view). Nevertheless, JEREMIAS, *Rediscovery*, 17-18, concedes: "This information from Cyril is of utmost importance.... it is the testimony of the Bishop of Jerusalem who would know by sight the holy places in Jerusalem including the ruins of Bethesda (for the five porticoes were in ruins in his day)".

⁽⁵⁵⁾ Jerome's version of the *Onomasticon*: "formerly it had five porticoes" (*quinque quondam porticus habuit*) (BALDI, *Enchiridion*, 456; TISCHENDORF, *Novum Testamentum Graece*, ad loc.).

⁽⁵⁶⁾ μετὰ γὰρ τὰς ἐκ κύκλῳ τέσσαρας μέσσην εἶχεν ἑτέραν (R. DEVREESE, *Essai sur Théodore de Mopsueste* [Città del Vaticano 1948] 324): "besides the four [porticoes], it had another in the middle".

⁽⁵⁷⁾ ἐν Ἱεροσολύμοις προβατικὴ τις ἦν κολυμβήθρα, καὶ νῦν ἐστὶν πέντε στοὰς εἶχε. νῦν γὰρ περιηρέθη τὰ πέριξ οἰκοδομήματα, "in Jerusalem there was a sheep pool, and it is still there; it had five porticoes. For now the surrounding buildings have been removed" (*Homily on the Sower* 15; PG 28, col. 164).

⁽⁵⁸⁾ Indeed, Jeremias himself seems hesitant about this approach. After arguing for the view, he quickly offers an alternative: "The ἔστιν, 'is', naturally could also be explained on the assumption that the tradition was

The evidence in fact is so one-sided — and impossible of harmonization — that it becomes necessary only to surmise how two of the writers have erred. On this we can but conjecture, of course, but there are several good possibilities. First, the Pilgrim of Bordeaux might have written in conscious imitation of the wording of John 5,2. His participial clause, *quinque porticos habentes*, follows John's text exactly — in both syntax and word order. In fact, even Eusebius does this, though he adds τὸ παλαιόν at the front of the clause to clarify that the porticoes had long since disappeared by AD 324. This is also similar to Theodoric's later comment that "the sheep pool which, *as it is written in the Gospel*, has five porticoes" (*piscinam probaticam... quae, sicut in evangelio scribitur, quinque porticus habet*)⁽⁵⁹⁾. Incidentally, Theodoric's testimony about the existence of the porticoes in his day is clearly wrong — as even Jeremias would admit — since the Persians had destroyed the pool well before his time, on May 20, 614⁽⁶⁰⁾.

Secondly, the error could have been a slip of the pen for either writer. Since both wrote in Latin, this is all the more likely (especially for the Pilgrim of Bordeaux). Note that the Pilgrim used the present active participle (*habentes*), just as John had done (ἐχουσα). As B. Fischer points out, "Latin verbs lack the perfect active participle and present passive participle.... If therefore the Greek participial construction is retained, the time relationship must often be sacrificed; otherwise one must alter the construction"⁽⁶¹⁾. Thus, a subconscious allusion to John 5,2 or a forgotten temporal particle might have caused the error. With reference to the anonymous writer, who uses a present indicative (*habet*) in describing the porticoes, this too could be a mental slip. I have noticed one modern writer, in fact, who has done a similar thing: "In Jerusalem there are two pools of water which can with confidence be as-

formulated before A.D. 70 and the present tense was retained unchanged after that date" (*Rediscovery*, 14, n.21; for a critique of this approach, see our discussion in the following section [II. 4]).

⁽⁵⁹⁾ BALDI, *Enchiridion*, 461 (italics added).

⁽⁶⁰⁾ *Rediscovery*, 21.

⁽⁶¹⁾ "Limitations of Latin in Representing Greek", in B.M. METZGER, *The Early Versions of the New Testament: Their Origin, Transmission and Limitations* (Oxford 1977) 366-367; cf. also R.W. MOORE, *Comparative Greek and Latin Syntax* (London 1962) 93.

sociated with Jesus' ministry The other pool is that of Bethesda, a pool with five porches or porticoes ..." ⁽⁶²⁾.

Thirdly, either writer might have been a charismatic propagandist (of a sort), intentionally promoting the veneration of a holy site. Both refer to the healing powers of the pool as present in their day. The fact that only these two authors refer to both the porticoes and the healings as though still with them casts an equally long shadow of incredulity over the one as over the other.

Finally, either writer could have been mistaken about the identity of the porticoes. *Porticus* in Latin, as *στοά* in Greek, is somewhat fluid in meaning ⁽⁶³⁾. Besides, apart from the term's range of meaning is the perennial psychological desire of Christian pilgrims to make the evidence fit the 'facts'. Jeremias mentions one late Greek writer in this connection:

The porticoes had long since disappeared. People did not even know any more what to make of the reference to them in John 5:2 [after AD 614]. Epiphanius Hagiopolita (A.D. 750-800) writes, '...the Sheep Pool which has five porticoes or rather five vaults; some also speak of raised seats' ⁽⁶⁴⁾.

As mentioned earlier, Theodoric's comment (a Latin author) also seems to fit this category. In Jeremias' approach, AD 614 is the *terminus ad quem* for the destruction of the porticoes, but it is distinctly possible that people did not know what to make of the references to the porticoes much earlier — perhaps even in the fourth century.

Nevertheless, even if it is granted that the porticoes no longer stood when Eusebius wrote, it is a *non sequitur* to argue that they *must* have collapsed when Titus razed Jerusalem. They could have been destroyed during the Bar Kochba revolt (AD 135) just as easily. Indeed, E.J. Vardaman argues that "If the Copper document [3Q15, i.e., the copper scroll] dates from around A.D. 100 as the most reliable authorities suggest, the Pool of Bethesda could have

⁽⁶²⁾ E.M. YAMAUCHI, *The Stones and the Scriptures* (New York 1972) 103-104. Though the grammar is more subtle, this sentence would certainly suggest to the archeological neophyte that the porticoes still stood (an impression quickly cleared up a few sentences later, however).

⁽⁶³⁾ S.v. *porticus* in *Oxford Latin Dictionary* (ed. P.G.W. GLARE) (Oxford 1982) and in *A Latin Dictionary* (ed. C.T. LEWIS – C. SHORT) (Oxford 1900); *στοά* in LSJ.

⁽⁶⁴⁾ *Rediscovery*, 21.

survived the destruction of Jerusalem [since the copper scroll also refers to the pool as still in existence] and this would explain the present tense in John 5:2, 'there *is* at Jerusalem'" (65).

We suggest four considerations, however, which seem to strengthen our contention that the porticoes were destroyed in 70. First, it is not at all certain that the copper scroll should be assigned a date of *c.* AD 100. Geza Vermes has recently argued, on the basis of compelling numismatic and paleographic evidence, that "*All the [Qumran] manuscripts, and consequently all the events to which they allude, are to be considered as prior to AD 68 [italics added]*" (66).

Second, Josephus states that, at the conclusion of the Jewish war, "Caesar ordered the whole city and the temple to be razed to the ground, leaving only the loftiest of the towers, Phasaël, Hippicus, and Mariamme, and the portion of the wall enclosing the city on the west..." (67) — a statement which, in the least, tends to confirm our view (68).

Third, recent archeological work in Jerusalem not only confirms Josephus' general topographical reliability, but also his accuracy with reference to the total devastation wrought by Titus' campaign (69).

Finally, and perhaps most significantly, Jeremias' (and van der Vliet's) identification of the pool of Bethesda has been brought into question because the columns which he believed supported the porticoes have now been identified as belonging to a much later period (70).

(65) "The Pool of Bethesda", *BT* 14 (1963) 29.

(66) *The Dead Sea Scrolls: Qumran in Perspective*, rev. ed. (Philadelphia 1981) 35; cf. also 29-39.

(67) *Jewish War* 7.1 (LCL translation by H. St. J. Thackeray).

(68) Josephus' comments about the district of Bezetha, where the pool was in all probability located (s.v. Βηθζαθά in BAGD; 'Bethesda' in the new *ISBEnc* 1:467-468 [by D.J. Wieand]) confirms more specifically the total devastation that this particular region suffered (*Jewish War* 2.530; 5.302).

(69) Cf. K. M. KENYON, *Jerusalem: Excavating 3000 Years of History* (New York 1967) 185-186; id., *Digging Up Jerusalem* (New York 1974) 250-256; E. M. YAMAUCHI, "Josephus and the Scriptures", *Fides et Historia* 13 (1980) 57-58; B. MAZAR, *The Mountain of the Lord* (Garden City, NY 1975) 14-15.

(70) DUPREZ, *Jésus*, 37-38, argues that the fragments of columns found near the pool belong to the Byzantine era and were not Herodian. He

In sum, the ‘intact’ view contains too many difficulties — and creates too many unnecessary contradictions among early witnesses, Josephus, and recent archeological findings — to be considered plausible.

4. *The Redactional View*

The fourth approach is the redactional one. According to this view, the author(s) composed the gospel at different stages, with some material being written before 70 (including 5,2), though the completed form of the gospel was not published until the 90’s. The ἔστιν in 5,2 was unaltered, even though no longer true, because the author — or a later editor — did not bother to change it.

Very few commentators actually apply this approach to John 5,2⁽⁷¹⁾. I suppose that this is due to the fact that the view did not become highly developed until the use of 5,2 for an early date had become a dead issue. Nevertheless, the redactional approach to the gospel as a whole now assumes the role of “the new critical orthodoxy”⁽⁷²⁾ in Johannine scholarship. Hence, we can safely assume that this particular application of the approach would have appeared in the literature more frequently if Theodor Zahn had not closed the coffin lid years ago on the evidence in 5,2.

Therefore, we need to fill in the gaps somewhat by making a quick overview of the redactional approach, followed by a critique with reference to John 5,2. In the least, a redactional view of John begins with two assumptions: (1) much of the material in this gospel goes back to a tradition before the fall of Jerusalem — perhaps to

makes perhaps too much of this, however, when he states, “L’archéologie ne permet donc pas d’affirmer l’existence [*sic*] des portiques” (38), for the absence of Herodian columns near the top of the excavation site does not necessarily argue that the pool never had porticoes, but, in keeping with our arguments above, that the porticoes were leveled in AD 70. Duprez’s view that the therapeutic centers in the caves nearby were the porticoes of John 5,2 involves too many problems (see n. 47).

⁽⁷¹⁾ But see JEREMIAS, *Rediscovery*, 14, n. 21; G. H. C. MACGREGOR, *The Gospel of John* (The Moffatt New Testament Commentary; Garden City, NY 1929) 167.

⁽⁷²⁾ ROBINSON, *Redating*, 263. What follows is a detailed critique of the multi-stage theory of the composition of John (263-277). Cf. also, analogously, D. A. CARSON, “Current Source Criticism of the Fourth Gospel: Some Methodological Questions”, *JBL* 97 (1978) 411-429.

one even more primitive than that found in the synoptics, and (2) the gospel has been worked over by at least two different hands — as is obvious by the group commendation in 21,24 — “This is the disciple who testifies of these things and who wrote them; and we know that his testimony is true”. These two assumptions are the starting gate from which several have raced in many directions. Some — especially the more conservative commentators — would concede only these two; others would argue that the gospel went through at least five stages over a period of fifty years or so⁽⁷³⁾. As Cullmann so pointedly remarked:

...indeed, the whole work has been dissected into a series of successive editions and redactions, each assigned to a particular author or a particular trend. In some theories one such redaction may even be diametrically opposed to the one which went before⁽⁷⁴⁾.

By way of critique⁽⁷⁵⁾, we may start with Cullmann's assessment. The fact that there is no unanimity as to the number of redactions or redactors, or what in the gospel belongs to which stage or hand, makes any pronouncements about John 5,2 much less than iron-clad. Indeed, Cullmann laments over “the countless solutions”, noting that “the most striking thing about them is their dissimilarity”⁽⁷⁶⁾. All that we can dogmatically affirm about redaction in John, it seems to me, are the two assumptions with which we began: there is pre-70 material here and 21,24 was added by a different hand. Of course, on a probabilistic level, much more can be said. Nevertheless, at this stage it might not be overly cautious to affirm with D.A. Carson the stance of a ‘probing agnosticism’⁽⁷⁷⁾.

Secondly, as so many of the older commentators have pointed out, the only explanation of the silence about Jerusalem's destruction must be that this gospel was written just before 70 or quite a bit

⁽⁷³⁾ BROWN, *The Gospel According to John*, I, XXIV–XL.

⁽⁷⁴⁾ CULLMANN, *The Johannine Circle* (Philadelphia 1976) 1.

⁽⁷⁵⁾ We cannot hope to give a full-blown critique of the redactional view of the gospel (but see n.72), for not only is such a task outside the scope of this study, but I also see evidence of redaction in John here and there, though nothing that compels a date of publication after 70.

⁽⁷⁶⁾ CULLMANN, *The Johannine Circle*, 3.

⁽⁷⁷⁾ “Current Source Criticism”, 429 (though Carson was speaking of *source* criticism, the point is valid for redaction criticism, too).

after it⁽⁷⁸⁾. Yet, in the redactional approach, both are affirmed. It would seem that the older writers' axiom effectively refutes any redactional view which posits a reworking of the material within ten to fifteen years after 70. As well, on psychological grounds, it is difficult to surmount the catalyst-like effect that the leveling of the temple would have on an evangelist who had already written much material for his gospel. The older view that the entire gospel was written much later than 70, on this score at least, is more commendable.

Thirdly, the whole pericope about the healing of the crippled man (5,1-9) is undoubtedly early⁽⁷⁹⁾, though the present form of v. 2 must have been written, according to sound redactional principles, at a much later stage. Increasingly, scholars are arguing that "the disciples took notes, recording in written form, during the *pre-Passion period*, various saying and teaching[s] of Jesus"⁽⁸⁰⁾. On the one hand, this pericope reads as though it came from just such a primitive diary⁽⁸¹⁾; yet on the other hand, the form in which v. 2 now appears must come from a stage when the author had already targeted a non-Palestinian audience as the recipients of the gospel (for he introduces the five-porched pool as though his audience were

(⁷⁸) As H. A. W. MEYER suggests, "...we must put the composition not *before* the destruction of Jerusalem ..., but a considerable time *after*; for if that catastrophe had been still fresh in the recollection of the writer, in the depths of its first impression, it could hardly, on psychological grounds, have escaped express mention in the book" (*John* [in MeyerK] 38). Yet Meyer's brilliance in principle becomes lackluster in application, for he dates the gospel in AD 80!

(⁷⁹) BROWN, *The Gospel According to John*, I, 209: "The factual details found in the introduction ... are very accurate. They betray knowledge of Jerusalem that militates against a late or non-Palestinian origin of the story". Cf. also J. L. MARTYN, *History and Theology in the Fourth Gospel* (New York 1968) 52-53; KLINGER, "Universality", 180-181; L. Th. WITKAMP, "The Use of Traditions in John 5,1-18", *JSNT* 25 (1985) 22.

(⁸⁰) D. A. CARSON, "Historical Tradition in the Fourth Gospel: After Dodd, What?", *Gospel Perspectives: Studies of History and Tradition in the Four Gospels*, vol. 2, (ed. R. T. FRANCE – D. WENHAM) (Sheffield 1981) 113; cf. his bibliography in n. 99 (141-142).

(⁸¹) Besides the data in v. 2, note also the references to the duration of the man's illness ('thirty-eight years') in v. 5, his notoriously curious response to Jesus' question in v. 7 (especially if v. 4 is athetized!), and the comment about it being the Sabbath in v. 9 (*contra* WITKAMP, "Traditions", 31).

unacquainted with it); and furthermore, v.2 indicates that he had already selected, in all probability, a largely Gentile audience (for the explanation of 'Bethesda' as an Aramaic/Hebrew name would be superfluous trivia to Palestinian Jews). The redactional approach to this verse, therefore, is its own refutation: how could a later editor of the gospel not bother to change the ἔστιν when he had reworked so much else in this verse? The explanation that best fits the evidence is that the final edition was also completed before AD 70.

Nevertheless, if we were to adopt Brown's five-stage redaction, we would admit that this pericope (and perhaps all of chapter 5) could have been in its final form in the third level of redaction — two stages before the final edition. The gospel as we know it, according to this view (including chapter 21), did not come into existence until the late 90's. But in that case, the third stage of redaction (= first published edition) would still have to have been written before 70. Consequently it is fair to ask: If John 5 was in polished form before AD 70, how would this contribute to the date of the final edition of the whole gospel? In response, three arguments can be advanced.

First is the psychological argument (see above): If the gospel were in a polished form before 70, would not the Jewish War virtually beg the evangelist to incorporate a few apologetic notes about Jerusalem's demise in the fourth edition — especially in chapter 5? The catalyst of AD 70 would seem to be so strong that all five of the redactional stages would in all probability have taken place before 70 or well after it. In Brown's reconstruction, however, (two stages before 70, three after; or, as we have modified it, three before, two after) we seem to be forced to the improbable conclusion that as prolific as the evangelist(s) of "the gospel of Jerusalem" was, his only real interest in the holy city was literary, not theological.

Second, there may well be a subtle link between chapter 5 and chapter 21 (the latter being added in the final stage of redaction). The group commendation of the veracity of the gospel in 21,24, if made by the Ephesian elders as has been traditionally supposed, implies that a largely Gentile audience was targeted for the final edition of the gospel. As Brown points out, "it is perfectly legitimate to speak of several aims of the Gospel" (I, LXVII), one of which would be to encourage Gentile Christians in their faith. It seems quite probable that the inclusion of Gentiles as a part of the

audience (implicit in both 5,2 and 21,24) would belong to the last stage of redaction.

Third is the textual argument: Kurt and Barbara Aland have argued that any theories of textual displacement or of more than one published edition of the fourth gospel fly in the face of the manuscript evidence:

...for purposes of textual criticism the gospel comprises twenty-one chapters in their present sequence of 1 through 21. It is only in this form, with the final chapter appended and in the present order of chapters, that the book is found throughout the manuscript tradition. Any editing, rearrangement, revision, and so forth it may have undergone must have occurred earlier, if at all⁽⁸²⁾.

If the textual argument is valid, then to posit more than one published edition of the gospel is unwarranted⁽⁸³⁾. And if stage three and stage four of the redactional process are expediently labeled "unpublished editions", not only are scientific controls thereby removed, but the rationale for an unpublished edition becomes difficult to discover.

We must nevertheless admit that these three arguments are merely suggestive and tentative. Hopefully, if 5,2 is brought back into the discussion over the date of this gospel, some of the dialogue will focus on this issue, sharpening the arguments on both sides.

5. *The Historical Present View*

The fifth and last opponent to the natural implications of John 5,2 is the historical present view. According to this approach, the author uses the present tense to refer to a place which is now just a memory. The historical present — as opposed to the customary present — is commonly used in the gospels to refer to a past situation. Thus, on the surface, this argument has much to commend it.

⁽⁸²⁾ K. and B. ALAND, *The Text of the New Testament*, trans. E. F. Rhodes (Grand Rapids-Leiden 1987) 292. This case (which the Alands make for several NT books) can especially be sustained for John since there are more papyri containing (portions of) the fourth gospel than any other NT book.

⁽⁸³⁾ Brown's argument that the probability of more than one edition of Jeremiah analogically demonstrates the probability of more than one edition of John (BROWN, *The Gospel According to John*, I, xxxix) breaks down because Jeremiah has textual evidence of more than one edition (MT vs. LXX) while John does not.

This view is far and away the most popular among the commentators — especially since Zahn expounded on its virtues so long ago. But a perusal of the commentaries will reveal a curious fact: almost none of the advocates of this approach call ἔστιν a historical present. Indeed, I have only seen Schnackenburg to refer to it by this name⁽⁸⁴⁾. Most speak of ἔστιν with such phrases as “the *vividness* of recollection [italics added]”⁽⁸⁵⁾, “*rhetorical vivacity* [italics added]”⁽⁸⁶⁾, “*vivid* memory [italics added]”⁽⁸⁷⁾, or “the Evangelist *envisages* [Jerusalem] as it was when he knew it [italics added]...”⁽⁸⁸⁾. Yet, when one checks the grammars he notices the same kind of description for the historical present. Burton says that this present “is used to describe *vividly* a past event in the present of which the speaker conceives himself to be [italics added]”⁽⁸⁹⁾. Blass–Debrunner, as well as Turner, speak of it as “common... in *vivid* narrative where the speaker imagines himself present [italics added]”⁽⁹⁰⁾. Dana and Mantey tell us that “the present tense is thus employed when a past event is viewed with the *vividness* of a present occurrence [italics added]”⁽⁹¹⁾. And, as we have suggested, apart from the customary present, there is no other category of usage to which a late-date advocate can appeal if he chooses a syntactical solution to the ‘problem’ of John 5,2.

Why then this reticence on the part of the commentators to label ἔστιν as a historical present? I think that, for many, there may be an underlying uneasiness with this label⁽⁹²⁾. They perhaps recognize that εἰμί is not well suited to the special nuances of the historical present. Yet if they describe the ἔστιν in John 5,2 with the same terms with which the grammarians refer to the historical

⁽⁸⁴⁾ SCHNACKENBURG, *The Gospel According to St John*, 2:460, n. 9.

⁽⁸⁵⁾ F. GODET, *Commentary on the Gospel of John*, trans. T. Dwight (New York 1886) 1:455.

⁽⁸⁶⁾ J. P. LANGE, *The Gospel According to John*, 180.

⁽⁸⁷⁾ A. T. ROBERTSON, *Word Pictures in the New Testament*, vol. 5: *The Fourth Gospel, the Epistle to the Hebrews* (Nashville 1932) 78.

⁽⁸⁸⁾ BRUCE, *The Gospel of John*, 121.

⁽⁸⁹⁾ BURTON, *Moods and Tenses* 9 (§ 14).

⁽⁹⁰⁾ TURNER, *Syntax*, 60. Cf. also BDF, 167 (§ 321).

⁽⁹¹⁾ DANA–MANTEY, *A Manual Grammar of the Greek New Testament*, 185.

⁽⁹²⁾ The commentators’ consistent rendering of ἔστιν as ‘is’, while at the same time translating obvious historical presents as though they were past tenses, reveals such an uneasiness.

present (and they use terms which cannot describe any other usage of the present tense), then we might say that a historical present by any other name still gives off the same scent.

What is the basis, then, for treating ἔστιν as a historical present in John 5,2? Generally, two arguments are advanced⁽⁹³⁾. First, the historical present is very common in the gospels — especially in Mark and John. According to Hawkins, there are 151 historical presents in Mark, 93 in Matthew, 9 in Luke, and 162 in John⁽⁹⁴⁾. Thus it should not surprise us that John would use ἔστιν in this manner when such an idiom is everywhere present in his gospel.

Secondly, John uses the imperfect ἦν in some of his topographical notes to describe places which continued to exist after 70. In 4,6, for example, he points out that, with reference to Sychar, “the well of Jacob *was* there”⁽⁹⁵⁾. But Jacob’s well was not destroyed in 70, nor at any time later; it is, in fact, one of the best established biblical sites in Palestine today⁽⁹⁶⁾. In 11,18 John says “now Bethany *was* near Jerusalem”⁽⁹⁷⁾. Yet, as far as we know,

⁽⁹³⁾ ZAHN, *Introduction to the New Testament*, 3:349, n.6, adds two other minor arguments. First, he attempts to draw a mild analogy between the present tense in Hebrews and ἔστιν in John 5,2, though he quickly moves on. Secondly, he refers to Josephus’ usage of καλέω in the present tense to refer to two places which had been destroyed in AD 70. In the first text, *Jewish War* 5.145 (ὃν καλοῦσιν Ὀφλαῖν), there can be no objection to the verb being a historical present. However, this is quite irrelevant to the situation at hand, for the verb is active and perfectly conforms with the pattern of historical presents found elsewhere. In the second text, *Jewish War* 5.149 (ὃς καλεῖται Βεζεθαῖ), Zahn shows a much closer parallel to John 5,2, for not only is Josephus more than likely (see BAGD for variants in spelling on the name Βηθζαθαῖ) discussing the same general location (i.e., the northeastern section of the city known as Bethesda; not the pool *per se*), but he also uses a *functional* copula. In my thinking, this is a better semantic approach to take for the historical present advocates. The fact, however, that only one slightly tapered parallel (which does *not* use εἰμί, nor a true copula) from a single source has been produced is most telling. Further, we have no argument with the fact that that portion of the city known as Bethesda continued to exist after 70, but that the porticoes of the pool of Bethesda stood after 70 is altogether a different matter.

⁽⁹⁴⁾ J. C. HAWKINS, *Horae Synopticae*, 2d ed. rev. (Oxford 1909) 143-149.

⁽⁹⁵⁾ ἦν... ἐκεῖ πηγή τοῦ Ἰακώβ.

⁽⁹⁶⁾ See YAMAUCHI, *The Stones and the Scriptures*, 102-103; A. M. HUNTER, *According to John* (London 1968) 51-52.

⁽⁹⁷⁾ ἦν δὲ ἡ Βηθανία ἐγγυὺς τῶν Ἱεροσολύμων.

Bethany was continuously inhabited “from c. the sixth century B.C. to the fourteenth century A.D.”⁽⁹⁸⁾. Thus, by analogy, we can no more force ἔστιν in 5,2 to prove the existence of a pool at the time of writing than we can force ἦν in 11,18 to prove the disappearance of a town when the gospel was published⁽⁹⁹⁾.

We will face these two arguments chiasmically. First, to suggest that the imperfects in John which transgress their normal temporal boundaries are analogous to ἔστιν as referring to past time is to throw the oranges into the apple bin. It is a puzzling thing to me how the use of one tense-form can be marshalled as primary evidence for the nuances of another. Yet many appeal to this argument, assuming that nothing else needs to be said to prove the point⁽¹⁰⁰⁾. To be specific, a basic problem with this approach is that although the imperfect and present tenses live somewhat parallel lives in their aspectual forces⁽¹⁰¹⁾, they are not altogether analogous in their temporal nuances. There is no historical, futuristic or gnomic imperfect, for example⁽¹⁰²⁾. But more fundamentally, even if we were to grant the possibility of an exact temporal parallel, it has

⁽⁹⁸⁾ S.v. “Bethany,” in *The New International Dictionary of Biblical Archaeology* (ed. E.M. BLAICKLOCK – R.K. HARRISON) (Grand Rapids 1983). See also J. WILKINSON, *Jerusalem as Jesus knew it: Archaeology as Evidence* (London 1978) 110-113.

⁽⁹⁹⁾ Cf. ZAHN, *Introduction to the New Testament*, 3:349, n.6. This is not to charge our author with carelessness in his use of Greek tenses; rather, the argument is meant to charge pre-70 advocates with hair-splitting on a bald head! Zahn adds the following parallels to this approach: “Josephus (*Bell.* v.1-4) regularly uses the imperfect with reference to buildings and even localities not affected at all or not essentially changed by the destruction of Jerusalem (e.g. the towers of Hippicus, Phasaël, and Mariamne, which Josephus himself tells us, *Bell.* vii. 1, 1, remained undamaged, τετράγωνος ἦν, etc.), the imperfect tense is used in the entire account (τρίτος ἦν λόφος, περιείχοντο, ἐκαλεῖτο [not only ὑπὸ Δαβίδου but also πρὸς ἡμῶν], ἐκαλοῦμεν, ἐκάλουν)” (ibid.).

⁽¹⁰⁰⁾ This is the mainstay in Zahn’s argument (ibid.), the only point in BULTMANN’s, *The Gospel of John*, 240, and the major concession in ROBINSON’s, *Redating*, 278.

⁽¹⁰¹⁾ In that both are generally linear tenses as opposed to punctiliar or perfective.

⁽¹⁰²⁾ Although it could be argued that this proves that the present tense is more elastic temporally than the imperfect — and hence quite capable of sustaining such a parallel — such an argument is fallacious since, in any given instance, we are not comparing a single nuance of one tense-form with multiple nuances of another tense-form.

not been drawn for us: the true parallel (negatively stated) to an imperfect making no comment about the present time would be a present tense saying nothing about the future time⁽¹⁰³⁾. The flip-side is equally bad for this approach: if the imperfect of εἰμί in one place means 'was — and still is' then the parallel to the present tense *cannot* be 'was — but is no longer'. A true parallel will not let ἦν live in the past *and* the present, while restricting ἔστιν just to the past. When a more accurate analogy is seen for these two tenses, then, the whole point becomes irrelevant — and, in fact, contradictory — to the position of the historical present advocates.

Nevertheless, although the imperfects in John 11,18 and elsewhere are inadmissible evidence, we might still wonder why the author does not change them to the present tense-forms. I think that there are at least three possible answers (and it is interesting that Zahn unwittingly spends much time arguing in the same direction we will go): (1) the distance of space, as well as time, could easily account for some of these imperfects; if Sychar had not been visited for thirty-five years or so, to say that "Jacob's well *was* there" is perfectly appropriate (such could also explain the reference to Bethany in 11,18); (2) John possibly "assimilates his topographical descriptions to the tense of the narrative"⁽¹⁰⁴⁾; this would easily account for the references in 18,1 and 19,41; and (3) if this gospel was published shortly before the fall of Jerusalem, the author might be using guarded language in these descriptions. This would especially fit 4,6 since Sychar was apparently evacuated in 66 or 67⁽¹⁰⁵⁾.

The other side of this last option might well explain why John used ἔστιν in 5,2 (especially since, as we have already noted⁽¹⁰⁶⁾, he

⁽¹⁰³⁾ This can easily be seen in the temporal parallels between the pluperfect and perfect tenses. Further, and more specifically, a healthy understanding of semantics will help one to see this inherently restrictive nature of grammar. The imperfects in 4,6; 11,18, etc., do not in fact refer to the present time at all — either by way of negative or positive implication. Indeed, the ἦν of 4,6 only tells us that Jacob's well was there when Jesus came to Sychar; that it still exists is proven by the archeologist's spade, not the grammarian's pen.

⁽¹⁰⁴⁾ ROBINSON, *Redating*, 278.

⁽¹⁰⁵⁾ See ALBRIGHT, *The Archaeology of Palestine*, 247-248; J. FINEGAN, *Light from the Ancient Past: The Archaeological Background of Judaism and Christianity* (Princeton 1959) 310-311.

⁽¹⁰⁶⁾ Cf. n. 103.

could just as honestly have used ἦν — and still have published the gospel before 70): if this gospel received its finishing touches just prior to publication in Ephesus (or somewhere outside of Palestine), one of the evangelist's most recent memories would have been of the pool of Bethesda, due to (a) its proximity to the temple as well as its possible employment for Christian baptisms, as several writers point out, and (b) if the author left Jerusalem during or just before the Jewish War broke out in 66, it would have been a rather short interval between his last glimpse of the pool and the publication of his gospel. In effect, he might have written ἔστιν because the pool formed a fresh and deeply imbedded picture in his mind's eye.

The second pillar on which this stands is more direct: ἔστιν is a historical present. Perhaps this argument will fare better than the 'imperfect' analogy. But, as we have already pointed out, only Schnackenburg explicitly labels ἔστιν as a historical present⁽¹⁰⁷⁾, though such an equation is almost certainly on everyone else's mind. It is not surprising then to find no defense of this point, only assumption.

Such an assumption, however, is another instance (as in the customary present view) of grammatical near-sightedness. The salient features of the historical present (apart from the obvious use of the present tense used to describe a past event), as the standard grammars point out, are (1) an action verb, (2) occurring in narrative and (3) used for the sake of vividness — as though the author were reliving the story⁽¹⁰⁸⁾. But since εἶμι could hardly be classified as anything

⁽¹⁰⁷⁾ A few others come very close, however, by defining ἔστιν as a present tense used to refer to something in the past; in addition to those mentioned earlier, see also I. KNABENBAUER, *Commentarius in Quatuor S. Evangelia*, vol. 4: *Evangelium Secundum Ioannem* (Paris 1898) 188; M.F. SADLER, *The Gospel According to St. John* (London 1910) 114; and, most remarkably, J.T. HARRIS, *The Writings of the Apostle John*, vol. 1: *The Gospels and Epistles* (London 1889), who speaks as though the pool were an event, not a place: "it is one of John's mannerisms in his Gospel not unfrequently to use the present tense for a past event in writing his history [italics added]" (71)!

⁽¹⁰⁸⁾ A few NT grammarians dispute the idea of vividness as the significance of this usage (see, e.g., S.M. REYNOLDS, "The Zero Tense in Greek", *WTJ* 32 [1969] 68-72; R. BUTH, "Mark's Use of the Historical Present", *Notes on Translation* 65 [1977] 7-13; R. ENOS, "The Use of the Historical Present in the Gospel According to Saint Mark", *The Journal of the Linguistic Association of the Southwest* 3 [1981] 281-298; C.D. OSBURN, "The Historical Present in Mark as a Text-Critical Criterion",

like an action verb, it is transparent that this verb does not readily carry such a nuance⁽¹⁰⁹⁾. This should give us our first clue as to the inherent weakness of the historical present approach.

In Hawkins' *Horae Synopticae*, the author lays out every historical present in the synoptic gospels and Acts — 216 in all⁽¹¹⁰⁾. From these raw data certain trends become evident: (1) every historical present in the list is in third person⁽¹¹¹⁾; (2) every

Bib 64 [1983] 486-500; and, more generally, K. T. RODEMEYER, *Das Praesens historicum bei Herodot und Thukydides* [Basel 1889]; P. KIPARSKY, "Tense and Mood in Indo-European Syntax", *FoundLang* 4 [1968] 30-57; and N. WOLFSON, "The Conversational Historical Present Alternation", *Language Journal of the Linguistic Society of America* 55 [1979] 168-182; against them, note in particular the following [all of whom see nuances of vividness for the historical present as well as other nuances]: E. SCHWYZER, *Griechische Grammatik*, vol. 2: *Syntax und syntaktische Stilistik* [rev. A. Debrunner; München 1953-60] 271-272; R. L. SHIVE, "The Use of the Historical Present and its Theological Significance" [unpublished Th. M. thesis, Dallas Theological Seminary, 1982]; and A. RIJKSBARON, *The Syntax and Semantics of the Verb in Classical Greek* [Amsterdam 1984] 22-25). Although the particular significance of the historical present is ancillary to our discussion, it is worth noting that (1) the NT grammarians who discredit the 'vividness' idea have worked almost solely in Mark, not in John, and (2) if a vivid reliving of the story is part of the significance of the historical present in John's gospel (so E. A. ABBOTT, *Johannine Grammar* [London 1906] 350 [§2482]; SHIVE, "Historical Present", 36-57; et al.), it might serve as a secondary and subtle argument that this gospel was written early since one's memory fades with time.

⁽¹⁰⁹⁾ However, I have seen only two writers make this point explicitly. H. A. STURZ, "Observations on the New World Translation", *Bible Collector* 7:27-28 (1971) 13: "Nowhere do the grammars include in their analyses the use of the verb 'to be' as a historical present"; and SHIVE, "Historical Present": "The historical present appears to be used only with action verbs. It has been suggested that this phenomenon never occurs with the copula in Greek. Clearly γίνομαι does occur in the historical present (Xenophon, *Anabasis* I.1.1.; Herodotus 1.102) yet here the verb is not functioning as a copula. Yet in an initial study several possible exceptions were found: Thucydides VI.52.2; Herodotus 1.101; and Aeschylus, *Persae* 447. Yet an examination of each seems to reveal that each communicates a fact that is true at the time of the author's writing and thus is not a historical present. Therefore it seems safe to conclude that this phenomenon is used only with action verbs" (19).

⁽¹¹⁰⁾ Based on Westcott and Hort's text.

⁽¹¹¹⁾ It does not necessarily follow, of course, that the first and second person could not be used as historical presents in some literature, but the nature of narrative is such that such occurrences would be rare. For

historical present is in the indicative mood⁽¹¹²⁾; and (3) εἰμί is not on the list⁽¹¹³⁾. Of the thirty or so NT grammatical works I have had occasion to glance at with reference to the historical present, not one identified any example of εἰμί as bearing such a nuance⁽¹¹⁴⁾. Furthermore, in my examination of John, I have found no such examples⁽¹¹⁵⁾. It is no wonder then that the historical present advocates are a bit clandestine in their descriptions of ἔστιν; this verb will simply not fit the mold of the historical present⁽¹¹⁶⁾.

possible examples, cf. GILDERSLEEVE, *Syntax of Classical Greek*, 1:86; and SCHWYZER, *Griechische Grammatik*, 2:271-272.

⁽¹¹²⁾ This would seem almost unnecessary to mention, except that the translators of the NASB on three occasions label participles as historical presents (προσελθόντες [aorist!] in Matt 15,12; ἐλθόντες [aorist!] in Mark 12,14; and, perhaps most remarkably, οὔσης [from εἰμί] in John 20,1)!

⁽¹¹³⁾ However, γίνεται does occur twice, though in these instances it is not functioning as a copula or predicate (Mark 2,15; 4,37).

⁽¹¹⁴⁾ Rarely, an example or two surfaces in the classical grammars (e.g., ἔστι in Aeschylus, *Persae* 447 mentioned in SCHWYZER, *Griechische Grammatik*, 2:271-272), but there is some doubt over whether these have been identified correctly (see SHIVE, "Historical Present", 19).

The only possible exception among NT grammars is ABBOTT, *Johanne Grammar*, where in an obscure footnote he mentions ἔστιν in John 6,14 (350, n.3 [§2482]). Yet, if Abbott views this as a historical present (and I am not certain that he does), such an identification would be clearly wrong, for the ἔστιν follows a declarative ὅτι, thus placing it in a different category altogether (see A. T. ROBERTSON and W. H. DAVIS, *A New Short Grammar of the Greek Testament* [New York 101958] 365 [§451 (a)]; cf. Matt 14,26; John 4,1; 5,13; 6,64; Acts 23,24 for examples of the present tense — sometimes even εἰμί — retained in indirect discourse).

⁽¹¹⁵⁾ In the Apocalypse, however, there might be an instance (ἔστιν in 16,21, so labelled by NASB), but it is capable of other explanations.

In a short study, J. J. O'ROURKE, "The Historic Present in the Gospel of John", *JBL* 93 (1974) 585-590, lists the historical presents in John's non-speech material. He has one instance of ἔστιν (1,19), in which he nevertheless recognizes that "some consider this as a simple present" (586, n. 6). Indeed, this is a poor example, for the evangelist says simply "this is the testimony of John" (αὕτη ἔστιν ἡ μαρτυρία τοῦ Ἰωάννου). As SHIVE, "Historical Present", 58, points out, "John is presently presenting the testimony of John the Baptist, thus this is not a historical event and therefore not a historical present". To say, "this is the testimony" is to say "this is what John said", or "this is the record of what John testified".

⁽¹¹⁶⁾ Even if an Aramaic original for the fourth gospel is supposed, this will not circumvent our argument, as C. C. TORREY, "The Aramaic Origin of the Gospel of John", *HTR* (1923) 334-335, points out with special reference to 5,2.

Therefore, although it has been rather popular to describe the ἔστιν of John 5,2 in the same manner in which grammarians describe the historical present, there is no sound linguistic basis for doing so.

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By way of concluding remarks, what we have attempted in this paper is the resuscitation of an old argument which had been left for dead several decades ago. As I mentioned in the introduction, *if* it could be demonstrated that the arguments against the *prima facie* evidence in John 5,2 were wrong, such a demonstration might well reveal — on this one strand of evidence — the inherent improbability that John's gospel was published after AD 70. I will be the first, however, to admit that the arguments advanced in this paper are not airtight. Nevertheless, I hope that we have shown that John 5,2 deserves greater consideration in future discussions over the date of this gospel.

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SOMMAIRE

L'article cherche à faire de Jn 5,2 (« Il y a une piscine à Jérusalem ») une preuve en faveur d'une datation antérieure à 70 pour l'Évangile de Jean. Il y a cinq itinéraires ouverts à quiconque désire contester le sens *prima facie* de ce verset: (1) le rédacteur s'est trompé: il ne savait pas que la piscine avait été détruite; (2) *estin* est un présent d'usage; (3) la piscine et/ou les portiques restèrent intacts après la guerre juive; (4) le verset appartient à une rédaction antérieure de l'évangile et le présent fut laissé par erreur dans l'édition finale; (5) *estin* est un présent historique. Chacune de ces alternatives a des manques, faisant de Jn 5,2 un indice interne important pour la date de l'évangile.

The Historical Background to the Conquest of Samaria (720 BC)

The chain of events leading to the conquest of Samaria by the Assyrians and the deportation of its people is delineated in 2 Kgs 17,3-6 as follows (*RSV*):

(3) Against him came up Shalmaneser king of Assyria; and Hoshea became his vassal, and paid him tribute. (4) But the king of Assyria found treachery in Hoshea; for he had sent messengers to So, king of Egypt, and offered no tribute to the king of Assyria, as he had done year by year; therefore the king of Assyria shut him up, and bound him in prison. (5) Then the king of Assyria invaded all the land and came to Samaria, and for three years he besieged it. (6) In the ninth year of Hoshea the king of Assyria captured Samaria, and he carried the Israelites away to Assyria, and placed them in Halah, and on the Habor, the river of Gozan, and in the cities of the Medes.

Only one Assyrian king is called by name in this passage (v. 3). It is, however, clear that the reference is to two different kings, Shalmaneser V and Sargon II. According to the latter's inscriptions he had deported 27,290 of the inhabitants of Samaria. The deportation of the Israelites to northern Mesopotamia and Media as mentioned in v. 6b is doubtlessly the same as that undertaken by Sargon after his campaign to the west in 720 BC. Thus, whereas v. 3 refers explicitly to Shalmaneser, v. 6b implicitly refers to Sargon II, his successor to the throne. One may ask where in the account the deeds of Shalmaneser come to an end and where those of his successor begin, or in other words, which of the two kings besieged and conquered Samaria and is referred to in vv. 5-6a.

I. Criticism of the Commonly Accepted Reconstruction of the Fall of Samaria

In his inscriptions Sargon II several times repeats that he conquered Samaria at the beginning of his reign. For many years scholars interpreted 2 Kgs 17,6 in the light of Sargon's claim in his inscriptions. Only a very few denied the authenticity of Sargon's

claim and assigned the fall of Samaria to Shalmaneser⁽¹⁾. A turning point in the direction of the research came when H. Tadmor published his detailed discussion of the inscriptions of Sargon⁽²⁾. He critically analyzed the royal annals and demonstrated that Sargon's first campaign to the West was conducted only in the second year of his reign (720 BC). During this campaign he defeated a coalition of states headed by the king of Hamath and recaptured the rebellious Syrian provinces of Arpad, Šimirra and Damascus. Subsequently he conquered Gaza and defeated Re'e, "the *turtānu* of Egypt", near Raphia. After the conclusion of the campaign he established new provinces at Hamath and Samaria, deported thousands of their inhabitants and rebuilt their centers.

Tadmor further suggested that the city of Samaria was conquered two years earlier by Shalmaneser V (722 BC), after a siege that lasted three years (724-722 BC). The episode of the long siege and conquest as described in 2 Kgs 17,5-6a and 18,9-10 refers to Shalmaneser; the campaign and conquest of the Assyrian king was also recorded in the Babylonian Chronicle (*urušamara'in iḫtepi*). Sargon's capture of the city was hardly a glorious operation — the glory for the military achievement should be attributed to his predecessor, Shalmaneser V⁽³⁾.

The hypothesis of the two conquests of Samaria — by Shalmaneser (722 BC) and by Sargon (720 BC) — was accepted by many scholars and, with minor variations, was recently defended by B. Becking in his detailed study of the sources referring to the fall of Samaria⁽⁴⁾.

(1) A. T. OLMSTEAD, "The Fall of Samaria", *AJSL* 21 (1904/5) 179-182; id., *Western Asia in the Days of Sargon of Assyria, 722-705 BC* (New York 1908) 45-47, n. 9; E. R. THIELE, *The Mysterious Numbers of the Hebrew Kings* (Grand Rapids 1965) 122-128.

(2) H. TADMOR, "The Campaigns of Sargon II of Assur", *JCS* 12 (1958) 33-40.

(3) TADMOR, "Campaigns", 36-40; id., "The Chronology of the First Temple Period", *The Age of the Monarchies: Political History* (A. MALAMAT ed.) (The World History of the Jewish People IV/1; Jerusalem 1979) 57-58; id., "Shalmaneser V", *Encyclopaedia Biblica* 7 (Jerusalem 1976) 713-715 (Hebrew).

(4) B. BECKING, *De Ondergang van Samaria* (Diss.; Utrecht 1985). For a list of works that followed the two-conquest hypothesis, see BECKING, *Ondergang*, 69-70, notes 241-242; S. DALLEY, "Foreign Chariotry and Cavalry in the Armies of Tiglath-Pileser III and Sargon II", *Iraq* 47 (1985)

However, this reasonable and well-constructed historical reconstruction is not free of uncertainty. Most obvious, of course, is the hypothesis of two separate conquests taking place within two (or three) years by two Assyrian kings, where the Bible and the Assyrian inscriptions refer to a single conquest. The problems arising out of the two-conquest reconstruction is outlined in the following seven points:

(A) Literary sources, reliefs and archaeological excavations agree that the conquest of non-submissive cities by the Assyrians was always an extremely destructive event. If Samaria was besieged for three years (with all the terrible effects of such a long siege) and then conquered by the Assyrians in 723/22 BC, one would hardly expect its weakened surviving inhabitants to be able to participate in a rebellion that broke out shortly afterwards. Yet according to Sargon's earliest inscription (the Aššur charter) — from his second year — the Samaritans participated in the alliance formed by the king of Hamath alongside the older provinces which had been annexed by Tiglath-pileser III⁽⁵⁾. Furthermore, the episode of the conquest of Samaria occupied a central place in Sargon's inscriptions composed in his later years, after he had conquered and annexed vast areas and had expanded the Assyrian empire to its maximal extent. It is thus clear that the king and his scribes regarded the former as a major event of the king's early years. The participation of the Samaritans in the rebellion of 720 BC and the place of the city's conquest in Sargon's inscriptions do not accord well with the assumption that Samaria was captured by Shalmaneser after a long siege and that Sargon had recaptured a weakened, ruined city.

33-36; N. NA'AMAN, "Historical and Chronological Notes on the Kingdoms of Israel and Judah in the Eighth Century BC", *VT* 36 (1986) 74; A. LAATO, "New Viewpoints on the Chronology of the Kings of Judah and Israel", *ZAW* 98 (1986) 217-218; H. DONNER, *Geschichte des Volkes Israel und seiner Nachbarn in Grundzügen*, 2 (Göttingen 1986) 314-315.

⁽⁵⁾ OLMSTEAD, *Western Asia*, 45-49; TADMOR, "Campaigns", 37, n. 137; H. W. F. SAGGS, "Historical Texts and Fragments of Sargon II of Assyria, 1. The Aššur Charter", *Iraq* 37 (1975) 11-20; DALLEY, "Foreign Chariotry", 34; J. D. HAWKINS, "The Neo-Hittite States in Syria and Anatolia", *The Cambridge Ancient History* 3/1 (J. BOARDMAN et al. eds.) (2nd rev. ed.; Cambridge 1982) 416-417.

(B) The excavators of Samaria attributed burned layers covering the later buildings of the Israelite period to the Assyrian conquest of the city⁽⁶⁾. The layer of debris of destruction that was found was, however, restricted to a relatively small area. Furthermore, the city walls continued to be in use over a long period of time⁽⁷⁾. Few remains have survived from level VII which the excavators attributed to the period of the Assyrian dominion in Samaria⁽⁸⁾. One wonders what would have been written in the archaeological report if the excavators had not been influenced by historical records. The relatively few signs of destruction revealed in Samaria are at variance with the findings from other cities that were captured — after much shorter sieges — by the Assyrians; signs of utter destruction are remarkably clear at all these sites. Lack of accord between the assumed three years of intensive siege and conquest as recorded in 2 Kgs 17,5-6a; 18,9-10 and the results of the archaeological excavations conducted at Samaria should warn us against a literal interpretation of the biblical text.

(C) The description of the capture of Samaria in the Nimrud prism opens thus⁽⁹⁾: “The Samaritans, whose [hearts]? became angry?” ([ŠĀ-šu-n]u? *ikmelūma*; or “who had repaid? [evil]?” - [lemutt]i? *igmelūma*)⁽¹⁰⁾ “against/to the king my [predecessor] ([ālik pāni]-ia) —

⁽⁶⁾ J. W. CROWFOOT-K. M. KENYON-E. L. SUKENIK, *The Buildings at Samaria* (London 1942) 110-112.

⁽⁷⁾ CROWFOOT-KENYON-SUKENIK, *Buildings*, 116-118; N. AVIGAD, “Samaria”, *Encyclopedia of Archaeological Excavations in the Holy Land* 4 (M. AVI-YONAH ed.) (Jerusalem 1978) 1046.

⁽⁸⁾ J. W. CROWFOOT-K. M. KENYON-E. L. SUKENIK, *The Objects from Samaria* (London 1957) 97-98; AVIGAD, “Samaria”, 1046.

⁽⁹⁾ C. J. GADD, “Inscribed Prisms of Sargon II from Nimrud”, *Iraq* 16 (1954) 179, 181-182 and pl. XLV-XLVI; TADMOR, “Campaigns”, 33-35; R. BORGER, *Textbuch zur Geschichte Israels* (K. GALLING ed.) (Tübingen 1968) no. 30; id., *Texte aus der Umwelt des Alten Testaments*, 1/4 (O. KAISER et al. ed.) (Gütersloh 1984) 382, note; DALLEY, “Foreign Chariotry” 36; H. SPIECKERMANN, *Juda unter Assur in der Sargonidenzeit* (FRLANT 129; Göttingen 1982) 349-350; BECKING, *Ondergang*, 51-55.

⁽¹⁰⁾ BORGER, *Geschichte Israels*, suggested transcribing *ik-me-lu-ma* and taking the verb as *kaṁālu* “to become angry” (CAD K 109). This rendering of the verb was also adopted by Spieckermann and Dalley. In this light we may restore [ŠĀ-šu-n]u at the beginning of line 28. Compare lines 249-250 of Sargon’s annals (A. G. LIE, *The Inscriptions of Sargon II King of Assyria* [Paris 1929] 38) PN *ana la našē bilti libbašu ikpuḍūma* with lines

not to endure servitude nor to bring tribute — and did battle, with the strength of the great gods, my lords, I fought with them...". Sargon explicitly refers here to the time of his predecessor, stating that the rebellion of the Samaritans had already started in the latter's reign. The reasonable inference is that he suppressed a rebellion that had already broken out in the time of Shalmaneser. Would Sargon's scribe have linked a rebellion started during the reign of Shalmaneser with the conquest of the rebellious city by his lord, if the rebellion was suppressed and the city conquered by the previous king?

Noteworthy also is the perfect accord between the words of Sargon's inscriptions, according to which the rebellion of the Samaritans was involved with "evil doing" and the cessation of the tribute, and the words of 2 Kgs 17,4, according to which Hoshea started conspiring with the king of Egypt and did not bring his yearly tribute. The agreement of the two sources gives further support to Sargon's inscription of the early stage of the rebellion and its suppression.

(D) The Babylonian Chronicle, in which the "ravaging" of Šamara'in under Shalmaneser is recorded, plays a central role in the currently accepted reconstruction of the capture of the city by the Assyrians⁽¹¹⁾. However, the text of the chronicle is organized throughout in a chronological order, with each and every event accurately dated within a specific year of the king of Babylonia and a transverse line marked to separate the years of reign. The "ravaging" of Šamara'in is included within the accession year of Shalmaneser and should accordingly be assigned to that year⁽¹²⁾.

25-28 of our passage *Samerinaya... ana la epēš ardūti [u la na]šē bilti [libbašun]u ikmelūma*. However, the verb *kamālu* appears only in religious contexts and has only the gods as subject (see BECKING, *Ondergang*, 52). As an alternative reading we may suggest here that [*lemutt*]i *ig-me-lu-ma* be transcribed, translating it in the light of West Semitic *gml r'h*, "to repay evil", "to treat badly". Compare Gen 50,15,17; 1 Sam 24,17; Isa 3,9; Ps 7,4[5]; Prov 3,30; 31,12. Admittedly, the reading has no parallel in the corpus of Akkadian literature.

⁽¹¹⁾ For the Babylonian Chronicle, see A. K. GRAYSON, *Assyrian and Babylonian Chronicles* (Locust Valley 1975) 69-87.

⁽¹²⁾ The dating of the "ravaging" of Šamara'in to 727 BC was taken by H. Winckler as an important piece of evidence against the identification of the city with Samaria. See H. WINCKLER, "Nachtrag", *ZA* 2 (1887) 351-352; id., *Die Keilschrifttexte und das Alte Testament* (E. SCHRADER ed.) (Berlin ³1903) 62. See further n. 27 below.

The claim that the conquest of Samaria should be assigned to the fourth/fifth year of Shalmaneser has no foundation in the listing of the episodes in the Babylonian Chronicle.

(E) The selection of the verb *hepû* to describe the capture of Samaria after a long siege is also problematic⁽¹³⁾. The author of the Babylonian Chronicle uses this verb to describe the plundering of vast areas or of groups of towns, whereas the capture of the larger cities is described by the verbs *šabātu*, *erēbu* and *kašādu*. Of the five references where the verb *hepû* appears, three refer to extensive areas (I,21 Bit-Amukanni; I,44 Bit-xxx; III,10-11 "from Rashi to Bit-Buranki"), one refers to two towns (II,24-25) and one mentions four towns (III,37-38). The last episode is described in the annals of Sennacherib by the words: "The cities which were in those districts I completely destroyed, I burned with fire and turned (them) into heaps of ruins"⁽¹⁴⁾. The fourth episode is also portrayed in detail in Sennacherib's annals and it is clear that an unconditional surrender rather than a siege or a breaking of the walls of the two cities had taken place⁽¹⁵⁾.

This analysis indicates that the verb *hepû* appears in the Babylonian Chronicle in the connotation of "to plunder" or "to ravage", and that other verbs were selected to designate the breaking of walls after a siege. It is thus questionable whether the Babylonian scribe would have used this verb to describe the breaking of the walls and the conquest of Samaria after a long siege. Another explanation for the notation "Šamara'in was ravaged" should be sought.

(F) There is a marked contrast between the text of 2 Kgs 17,4, according to which Hoshea was arrested and imprisoned before the siege of Samaria, and the text of v. 6 (and 18,10), according to which the city was captured in the ninth year of Hoshea. The assumption that Samaria was conquered in Hoshea's ninth year involves denial of both the sequence of events as described in 2 Kgs 17,3-6 (on this problem, see below) and the explicit statement that Hoshea was imprisoned before the beginning of the siege of Samaria.

⁽¹³⁾ The verb *hepû* in this context was formerly discussed by DALLEY, "Foreign Chariotry", 33, 36 and by BECKING, *Ondergang*, 43.

⁽¹⁴⁾ D. D. LUCKENBILL, *The Annals of Sennacherib* (Chicago 1924) 38, IV,44-46.

⁽¹⁵⁾ LUCKENBILL, *Annals*, 25-26, I, 54-56.

(G) The conquest of Samaria is described thus in v. 6: "In the ninth year of Hoshea the king of Assyria captured Samaria, and he carried the Israelites away to Assyria, and placed them in Halah...". All scholars agree that Sargon is the king who deported the Samaritans. Thus, according to the two-conquest hypothesis two different kings, Shalmaneser and Sargon, are referred to in this verse under the title "the king of Assyria". Accepting the hypothesis necessarily involves the assumption that the author of the passage lived a long time after the conclusion of the events and was unaware of the historical reality to the extent that he combined the actions of two Assyrian kings in one sentence.

In light of the many problems involved in the commonly accepted reconstruction of the siege and fall of Samaria, a fresh examination of the data is desirable. The biblical and Assyrian sources are examined in this article in order to clarify their structure and contents and try to suggest a better description of the chain of events culminating in Sargon's conquest of the city in 720 BC.

II. The Conquest of Samaria by the Assyrians — A Suggested Reconstruction

Our point of departure is the text of 2 Kgs 17,3-6. V. 3 states that Shalmaneser "came up" (*'ālāh*) against Hoshea and that the latter became his vassal. Since this episode is at the beginning of the description one would naturally assume that it describes an event during the early years of Shalmaneser's reign, prior to the episodes delineated in vv. 4-6.

Strangely enough the plain and clear sequence of events in vv. 3-6 was discredited by many scholars, who suggested that vv. 3-4 and vv. 5-6 are two parallel accounts of the same historical event, originating from different archives. The assumption that two different sources were combined in vv. 3-6 was first suggested by H. Winckler, mainly on historical grounds⁽¹⁶⁾. He suggested that vv. 3a and 5a refer to one and the same episode — Shalmaneser's campaign against Hoshea in his later years. He thus claimed that vv. 3-4 depend on archival documents of the Northern Kingdom

(16) H. WINCKLER, "Beiträge zur Quellenscheidung der Königsbücher", *Alttestamentliche Untersuchungen* (Leipzig 1892) 16-25.

whereas vv. 5-6 (and also 2 Kgs 18,9-11) were extracted from the archives of the Kingdom of Judah. Although Winckler himself later abandoned this hypothesis⁽¹⁷⁾, other scholars have followed this line of research. There are, of course, many variations in their analysis of the text, but they all confine the events described in vv. 3-6 to a single historical episode which took place in Hoshea's later years⁽¹⁸⁾.

When examining vv. 3-6 we must first of all note that the chain of events as related there is reasonable and that there are no stylistic or linguistic differences between the two assumed sources. The passage is written in the same verbal pattern as many other passages in the book of Kings, including the other episodes that refer to the campaigns of Assyrian kings against the kingdoms of Israel and Judah (2 Kgs 15,19-20.29-30; 18,13-15; cf. Isa 7,1). First comes a verb in the *qaṭal* form and subsequently there appear a series of verbs, all in the *wayyiqṭol* form. The two-sources one-episode hypothesis is not supported by textual criteria and is based only on a preconceived reconstruction of the historical chain of events. Methodologically (and, in my opinion, also historically), it is better firstly to try to follow the sequence of events as related in 2 Kgs 17,3-6.

First we must look for the historical background of Hoshea's early act of disobedience as related in v. 3. Since this act was directed against Shalmaneser, we may well assume that it refers to the time of unrest that followed the death of Tiglath-pileser III, the founder of the Assyrian empire, in 727 BC⁽¹⁹⁾. Rebellions that broke out following death of the founders of empires are so well known that it is redundant to look for examples. It is however worth noting that similar rebellions broke out several times, immediately after the

⁽¹⁷⁾ WINCKLER, in *KAT* (n. 12), 269.

⁽¹⁸⁾ BECKING, *Ondergang*, 25-29, with earlier literature in 24, n.93; A. ŠANDA, *Die Bücher der Könige*, 2 (EHAT 9/2; Münster 1912) 217; M. REHM, *Das zweite Buch der Könige* (Würzburg 1982) 167; G. HENTSCHEL, *2 Könige* (Die Neue Echter Bibel; Würzburg 1985) 77; LAATO, "New Viewpoints", 217-218.

⁽¹⁹⁾ C. F. LEHMANN(-HAUPT), "Menander und Josephos über Salmanassar IV", *Klio* 2 (1902) 128-129, 466-472; H. GOEDICKE, "The End of So, King of Egypt", *BASOR* 171 (1963) 64 (but see id., "727 vor Christus", *WZKM* 69 [1977] 4); H. J. KATZENSTEIN, *The History of Tyre* (Jerusalem 1973) 226.

deaths of later Assyrian kings, with the south-western frontier of the Assyrian empire always being an important center for unrest and revolts (e.g. after the death of Shalmaneser V⁽²⁰⁾, Sargon II⁽²¹⁾, Sennacherib⁽²²⁾ and Esarhaddon⁽²³⁾). It should also be recalled that Tiglath-pileser's last campaigns to the West were conducted in the years 734-32 and that in his last years he stayed in Babylonia and Assyria and, as far as we know, did not return to the conquered south-western regions. The surviving kingdoms thus had enough time to negotiate and conspire against Assyria. We may conclude that v. 3 most probably refers to unrest and perhaps even rebellion that broke out in the West upon the death of the great emperor and the accession of his son, Shalmaneser V.

In this context we may further note the prophecy of Isaiah (14,29-32) that echoes the hopes of the Philistine kingdoms, as the news broke of the death of their subjugator, possibly Tiglath-pileser⁽²⁴⁾. Far-sighted politicians like Isaiah, who warned against vain hopes at this delicate moment, were rare among the rulers of his time. Hoshea was a typical short-sighted politician and like the kings of Philistia he nourished a vain hope for a turn of fortune following the breaking of "the rod which smote".

⁽²⁰⁾ OLMSTEAD, *Western Asia*, 31-35, 40-41, 43-51; TADMOR, "Campaigns", 37-38; id., "Philistia under Assyrian Rule", *BA* 29 (1966) 90-91; HAWKINS, in *CAH* 3/1, 416-417; BECKING, *Ondergang*, 56-63.

⁽²¹⁾ For the rebellion that broke out upon the death of Sargon on the battlefield in 705 BC, see B. LANDSBERGER, *Sam'al* (Ankara 1948) 80-82; TADMOR, "Campaigns", 97 and notes 311-313; N. NA'AMAN, "Sennacherib's 'Letter to God' on his Campaign to Judah", *BASOR* 214 (1974) 32-34; HAWKINS, in *CAH* 3/1, 422, 426-427.

⁽²²⁾ For the murder of Sennacherib and the western campaign of Esarhaddon in his second year (679 BC), see H. HIRSCHBERG, *Studien zur Geschichte Esarhaddons König von Assyrien (681-669)* (Ohlau in Schlesien 1932) 5-16, 42-45; A. SPALINGER, "The Foreign Policy of Egypt preceding the Assyrian Conquest", *Chronique d'Égypte* 53 (1978) 22-33, 41-43; S. PARPOLA, "The Murderer of Sennacherib", *Death in Mesopotamia* (B. ALSTER ed.) (Mesopotamia 8; Copenhagen 1980) 171-181, with earlier literature.

⁽²³⁾ A. SPALINGER, "Assurbanipal and Egypt: A Source Study", *JAOS* 94 (1974) 316-325, with earlier literature; A.K. GRAYSON, "The Chronology of the Reign of Ashurbanipal", *ZA* 70 (1980) 244-245.

⁽²⁴⁾ H. WILDBERGER, *Jesaja*, 2. *Jesaja* 13-27 (BK 10/2; Neukirchen-Vluyn 1978) 573-579, with earlier literature.

Further evidence for the events of 727 BC may presumably be derived from the account of Menander as related by Josephus (*Ant.* IX 283-287)⁽²⁵⁾. Menander recounted the history of Tyre during the reign of Eloulaios, the sequence of events in his account being somewhat parallel to the chain of events in the history of Hoshea (2 Kgs 17,3-4). According to Menander's description Selampsas (in the Latin text Salmanasar) king of Assyria invaded Phoenicia and "made a treaty" with the Phoenician cities. The Tyrians, however, did not submit to him, and he came back and attacked the city of Tyre. When he retreated he placed guards on the mainland and blockaded the isle of Tyre for five years.

Common to the histories of Eloulaios, the king of Tyre, and Hoshea are the two-stage encounters with Selampsas/Shalmaneser⁽²⁶⁾. The first stage related by Menander may well be combined with the campaign of 727 BC, a campaign that was designed to pacify the western front, quell all possible rebellion and manifest the hegemonic status of Assyria in all areas up to the border with Egypt. The second stage may possibly be synchronized with Hoshea's conspiracy with Egypt and the withholding of the tribute (2 Kgs 17,4) in c. 724/23 BC (see below).

The above reconstruction also fits the account of the Babylonian Chronicle, according to which Shalmaneser "ravaged (*ihtepe*) Šamara'in" in his accession year. Assuming that the city of Šamara'in is indeed Samaria⁽²⁷⁾, we may suggest that soon after his

⁽²⁵⁾ For an analysis of the passage, see LEHMANN(-HAUPT), "Menander", 125-140; KATZENSTEIN, *History*, 222-230, with earlier literature.

⁽²⁶⁾ LEHMANN(-HAUPT), "Menander", 128-129, 466-472; ŠANDA, *Könige*, 212-213; KATZENSTEIN, *History*, 222-230.

⁽²⁷⁾ Several arguments raised by WINCKLER, "Nachtrag", 351-352; id., in *KAT* (n. 12) 62, against the identification of Šamara'in (or Šabara'in) with Samaria are worthy of consideration. Winckler dated the "ravaging" of the city to the accession year of Shalmaneser (727 BC), thus commenting that "die betreffende sta[d]t wurde im antrittsjahre Sargon's zerstört, während II Reg. 17, 5 deutlich sagt, dass Sargon Samaria drei jahre lang belagert habe". He further noted that the Babylonian Chronicle reports only events that were of interest to the Babylonians, which can hardly be said of the conquest of Samaria. Indeed, no western place is mentioned in the first part of the Chronicle, up to the time of Esarhaddon, with the exception of the land of Tabal, where Sargon was killed in battle. The chronicler described only the Assyrian-Babylonian-Elamite relations; the city of Samaria is an exception within this geographical region. Finally,

accession, when news of unrest and disobedience in the western vassal states reached Assyria, the new king immediately sent his troops to pacify the region. He himself may have participated in the campaign, although this cannot be verified. The extent of the unrest is not clear, but it was certainly limited and included Israel, the Phoenician coast and possibly also the Philistine kingdoms (Isa 14,29-32). Shalmaneser's immediate reaction was successful: all the disobeying kings immediately surrendered and paid their tribute; Samaria, possibly the center of resentment, was ravaged. How long the entire operation lasted cannot be established, but it certainly did not take long. According to the Eponymic Chronicle, Shalmaneser was in Assyria during the first year of his reign, so that even if he himself commanded the operation he was soon able to return to his own country.

The next stage should be dated c. 724/23 BC and is recounted in 2 Kgs 17,4: Hoshea sent envoys to "So, king of Egypt" and withheld his tribute. The identity of the king of Egypt is disputed among scholars⁽²⁸⁾. Of the various solutions raised so far for *sô'*, the suggestion to regard it as a title, as an inaccurate rendering of the Egyptian appellation for "king" (*nj-šw-t* > *nšw-t* > *nšw*), seems preferable⁽²⁹⁾.

Winckler correctly asked: "was ist das von Sargon eroberte Samirina, wenn Samaria schon 5 jahre vorher fiel?", "Nachtrag", 352.

Winckler's claim that Šam/bara'in must be sought in Babylonia may be sustained by the Assur Ostrakon, which refers to the deportation of captives of Bit-Adini (a Babylonian toponym) by Ululai, i.e. Shalmaneser V (H. DONNER-W. RÖLLIG, *Kanaanäische und Aramäische Inschriften* [Wiesbaden 1964] no.233, line 15). It is thus evident that the Assyrian king or his commander conducted a campaign into Babylonia, a campaign that would otherwise have been left out of the Babylonian Chronicle.

However, no city by the name Šam/bara'in is so far known in Babylonia, whereas there is a perfect accord between this name and the name of Samaria in the Babylonian-Aramaic tradition (TADMOR, "Campaigns", 39-40; BECKING, *Ondergang*, 42, with earlier literature in n.151). In the present state of our knowledge and in spite of the above observations, we must conclude that the city of Samaria is the one mentioned in the Babylonian Chronicle.

⁽²⁸⁾ See recently D.L. CHRISTENSEN, "The Identity of 'King So' in Egypt (2 Kings XVII 4)", *VT* 39 (1989) 140-153, with earlier literature.

⁽²⁹⁾ H. DONNER, "The Separate States of Israel and Judah", *Israelite and Judaean History* (J.H. HAYES-J.M. MILLER eds.) (London - Philadelphia 1977) 433; R. KRAUSS, "Sô', König von Ägypten - ein Deutungsvorschlag", *MDOG* 110 (1978) 49-54.

R. Krauss drew attention to the analogical title of *t3-ḥmt-nj-šw-t* “the king’s wife”, i.e. “queen”, which was likewise regarded in the Book of Kings as a personal name (1 Kgs 11,19)⁽³⁰⁾. Another case of the rendering of the Egyptian title *t3-ḥmt-nj-šw-t* as a personal name (Daḥamunzu) is known from the annals of Shuppiluliuma, King of Hatti⁽³¹⁾. There are quite a number of instances — in the Bible (Arawna/Awarna)⁽³²⁾, in the Amarna letters⁽³³⁾ and in the Assyrian royal inscriptions⁽³⁴⁾ — where foreign titles or designations of officials are taken as proper names. The interpretation of the “name” *sô*’ as a title is well supported by these examples. It is thus clear that the identity of the Egyptian king who tried to form an anti-Assyrian alliance can be clarified only by way of historical analysis.

We suggest that the conquest of Samaria and the quelling of the rebellion — which started by negotiations with an Egyptian king and the withholding of tribute (17,4a) — should be assigned to Sargon II. Since Egypt was involved in the rebellion against Sargon and an Egyptian commander with an army was sent to the Philistine coast to support the rebels, we may well assume that the Egyptian king who sent the task force in 720 BC is the same king of Egypt mentioned in 2 Kgs 17,4. In the period of the Sargonids, it is only the Ethiopian rulers of the 25th Dynasty who are known to have sent their troops across the Sinai peninsula to fight the Assyrians⁽³⁵⁾. Thus Pi(ankhy), who according to several scholars in-

⁽³⁰⁾ KRAUSS, “Sô’, König von Ägypten”, 51-52.

⁽³¹⁾ W. FEDERN, “Daḥamunzu KBo V 6 iii 8”, *JCS* 14 (1960) 33.

⁽³²⁾ P.K. McCARTER, *II Samuel* (AB 9; Garden City 1984) 512; S. ABRAMSKY, “The Attitude toward the Amorites and Jebusites in the Book of Samuel: Historical Foundation and Ideological Significance”, *Zion* 50 (1985) 55-56 (Hebrew). For another suggestion, see A. CODY, “Le titre égyptien et le nom propre du scribe de David”, *RB* 72 (1965) 381-393.

⁽³³⁾ W.F. ALBRIGHT, “Cuneiform Material for Egyptian Prosopography 1500-1200 BC”, *JNES* 5 (1946) 14, no. 16, 20-21, no. 53.

⁽³⁴⁾ R. BORGER, “Assyriologische und altarabistische Miscellen”, *Or* 26 (1957) 8-10. For further parallels, see W. EILERS, “Zur Funktion von Nominalformen”, *WO* 3 (1964/66) 137-140.

⁽³⁵⁾ A. Spalinger has pointed out that the Sargonids regarded the Nubian monarchs of the 25th Dynasty as foreign conquerors who had imposed themselves on the local princes. Thus there is a clear distinction in their presentation between the local and the Kushite kings. See A. SPALINGER, “The Year 712 BC and its Implications for Egyptian History”, *Journal of the*

vaded Lower Egypt in 724-22 BC and conquered it⁽³⁶⁾, was probably the king who conspired with Hoshea and later sent his commander with a task force to support the anti-Assyrian rebellion⁽³⁷⁾.

When Shalmaneser heard of the conspiracy he sent troops — under his own or his general's command — and ordered the conspirator to appear before him. Hoshea did so and "the king of Assyria shut him up, and bound him in prison" (v. 4b). In a somewhat similar situation Josiah appeared before his overlord, Necho king of Egypt, and was slain (2 Kgs 23,29). Hoshea was probably deported to Assyria and his subsequent fate remains unknown. The last king of the Northern Kingdom thus disappeared from the historical arena (723/22 BC). Whether the city of Samaria was besieged or the countryside plundered at that time cannot be established (see n. 45 below).

Shalmaneser died shortly afterwards and his heir to the throne, Sargon II, met serious opposition at home; he was able to put down the rebellion only with great difficulty. All Assyrian military operations in the West necessarily came to an abrupt end. Only in his second year (720 BC) was Sargon able to conduct a campaign to the West. At Qarqar he defeated a coalition of former Assyrian provinces and vassal states and won a second battle against an Egyptian task force near Raphia. His hands were then free to punish the Samaritans who had participated in the anti-Assyrian coalition

American Research Center in Egypt 10 (1973) 95-101; id., "Esarhaddon and Egypt: An Analysis of the First Invasion of Egypt", *Or* 43 (1974) 307-308, 320-326; N. NA'AMAN, "Sennacherib's Campaign to Judah and the Date of the LMLK Stamps", *VT* 29 (1979) 65.

⁽³⁶⁾ CHRISTENSEN, "King So", 146-153, with earlier literature.

⁽³⁷⁾ The appearance of black soldiers in Assyrian reliefs depicting the campaign of Sargon II to the Philistine coast in 720 BC supports and corroborates the assumption that the Egyptian army was dispatched by a king of the 25th (Kushite) Dynasty. See E. UNGER, "Äthiopier in assyrischer Darstellung", *RLA* 1 (1932) 310 and pl. 38b, with earlier literature; id., "Gabbutunu", *RLA* 3 (1957-71) 129-130; M. EL-AMIN, "Die Reliefs mit Beischriften von Sargon II in Dür-Sharrukin", *Sumer* 9 (1953) 35-36; M. WÄFLER, *Nicht-Assyrer neuassyrischer Darstellungen* (AOAT 26; Neukirchen-Vluyn 1971) 33-34, with earlier literature on p. 27, n. 102. In n. 142 Wäfler noted that "Der Zyklus Horsaabad... der die Schlacht von Rapihu darstellen dürfte — zeigt, sowohl noch erkennbar — ausschließlich Äthiopier". A similar conclusion, based on entirely different foundations, was reached by J.C. TREBOLLE, "La caída de Samaria. Crítica textual, literaria e histórica de 2 Re 17,3-6", *Salmanticensis* 28 (1981) 147-149, 152.

that fought him at Qarqar and he besieged and conquered the city of Samaria.

This is the background against which we should interpret 2 Kgs 17,5-6. V.5 "The king of Assyria invaded all the land and came to Samaria and... besieged it". This "king of Assyria" is no other than Sargon II and the verse refers to his western campaign of 720 BC. The invasion of "all the land" accurately reflects the wide scope of the Assyrian campaign, which had fateful consequences for the Northern Kingdom. In its importance for Israelite history it may be compared to Tiglath-pileser III's campaigns of 734-32 BC and to Sennacherib's campaign of 701 BC, both reported in the book of Kings (2 Kgs 15,29; 16,9; 18,13-16). The sequence of events as described in 17,5-6 (invasion of the land, siege and conquest of Samaria) is identical with the sequence of events as inferred from Sargon's inscriptions. It is thus clear that the reference in these verses was to the same king, i.e. Sargon II.

The Deuteronomistic Historian, who described the fall of the Northern Kingdom in 2 Kgs 17,3-6, in his sources had most probably found only one name of an Assyrian king (i.e. Shalmaneser). Therefore he erroneously assigned to him (though only implicitly) all the operations associated with the fall of Samaria. The campaigns and operations of the two Assyrian kings were combined by him in correct chronological order, but he was unable to draw a line between them due to the lack of clarity in his sources. We may conclude that "the king of Assyria" in vv.3-4 is Shalmaneser whereas the king in vv.5-6 is Sargon, his successor.

The description of the conquest of Samaria in the annals of Sargon is damaged; it may be partly restored on the basis of parallel accounts⁽³⁸⁾. In his detailed display inscription Sargon wrote thus:

⁽³⁸⁾ For restoration of the badly mutilated passage in the Khorsabad annals, see TADMOR, "Campaigns", 33-35. The similarity of this passage (lines 11-17) with the description of the conquest of Samaria on the Nimrud prism (IV,25-41) is clear and should not be questioned (contra BECKING, *Ondergang*, 44-47). The following broken episode in the annals (line 18) also has a parallel passage on the Nimrud prism (IV,46-49). R. BORGER, *Texte aus der Umwelt*, 382, has recently suggested restoring [ki-s]ur-re in line 46 of the prism ("I opened the sealed off border of Egypt"; see CAD K 153a). In this light we may suggest restoring m[i'-šir māt Mušur] in the annals. This may well have been a play of words (mišir - Mušur), similar to the play of words Re'e - rē'i in line 55 of the annals (see R. BORGER, "Das Ende des ägyptischen Feldheern Sib'e = סִיבָּה", *JNES* 19 [1960] 53).

"I besieged and captured the city of Samaria" ⁽³⁹⁾. In the Nimrud prism on the other hand no siege is mentioned, only a fight against the Samaritans ⁽⁴⁰⁾. In a display inscription Sargon wrote: "I plundered the city of Shinuhtu, the city of Samaria and the whole land of Bit Humri" ⁽⁴¹⁾. In several pavement inscriptions he described himself as "conqueror of the city of Samaria and the whole land of Bit Humri" ⁽⁴²⁾; and in a bull inscription he is portrayed as "(he) who overthrew the city of Samaria (and) all the land of Bit Humri" ⁽⁴³⁾. Siege operations are not emphasized in Sargon's inscriptions and no king of Israel is ever mentioned. It is thus evident that there was no king either in Samaria or in any other Northern Israelite city at that time.

S. Dalley has suggested that the Samaritans were treated favourably by Sargon after the capture of the city and the annexation of the land in 720 BC ⁽⁴⁴⁾. Her analysis of the Assyrian documents fits well into the historical reconstruction suggested here. It is not clear whether Samaria was severely damaged in the course of its conquest; the scanty archaeological evidence hardly supports the claim of overall destruction. We may rather assume a continuity of urban life and rapid reconstruction of the city under the Assyrians, when Samaria became the capital of the province of Samaria.

How should we interpret the data about three years of siege and the capture of Samaria in the ninth year of Hoshea? It is these data that led so many scholars to believe that Shalmaneser conquered Samaria, in disregard of the claim to the contrary in contemporary inscriptions of Sargon and the contradictory statement of v. 6b, which has the same subject as v. 6a ("the king of Assyria") but indisputably refers to Sargon. Are these really historical data

⁽³⁹⁾ H. WINCKLER, *Die Keilschrifttexte Sargons nach den Papierabklatschen und Originalen neu herausgegeben*, 1 (Leipzig 1899) 100-101, line 23; D.D. LUCKENBILL, *Ancient Records of Assyria and Babylonia*, 2 (Chicago 1927) § 55.

⁽⁴⁰⁾ GADD, "Prisms", 179-180, lines 29-30. For further literature, see note 9 above.

⁽⁴¹⁾ WINCKLER, *Sargon*, 82-83, line 15; LUCKENBILL, *ARAB* 2, § 80.

⁽⁴²⁾ WINCKLER, *Sargon*, 2, pl. 38-40, lines 31-32; LUCKENBILL, *ARAB* 2, § 99.

⁽⁴³⁾ D.G. LYON, *Keilschrifttexte Sargons Königs von Assyrien* (Leipzig 1883) 40-41, line 21; LUCKENBILL, *ARAB* 2, § 92.

⁽⁴⁴⁾ DALLEY, "Foreign Chariotry", 33-42.

derived directly from archival sources? In my opinion, the answer to this crucial question is negative. The three years of siege (v. 5b) and the date in v. 6a are both historical deductions by the Deuteronomistic Historian, who in his sources had found a datum that Samaria was besieged and conquered by the Assyrians three years *after* its rebellion and the imprisonment of its king⁽⁴⁵⁾. He mistakenly interpreted the datum to mean that the city fell — after three years of siege — in Hoshea's last year, thus combining his last year with the fall of Samaria.

A short note on the author of the passage is desirable for a better understanding of his mistaken historical deduction. It must be realized that he was in the first place an historian struggling with the source material available to him, trying to write a history of Israel according to his best understanding of past events and in accordance with his guiding historiographical concepts. He lived long after the conclusion of the events he portrayed and thus he depended heavily on his source material⁽⁴⁶⁾. The results of his interpretation of the sources may be read and appreciated today, in the light of many sources not available to him. Side by side with admiration for his achievement as an historian we must also remember that his work, like that of all human beings, is not free of mistakes caused by misinterpretation of source material⁽⁴⁷⁾.

⁽⁴⁵⁾ One may ostensibly suggest that Shalmaneser laid siege to Samaria immediately after the imprisonment of Hoshea and that the three years of siege (v. 5b) were counted from this early siege (723/22) and until Sargon's conquest (720). According to this hypothesis, v. 5 refers to Shalmaneser and v. 6 refers to Sargon. It is better however to assume that the two verses (5-6) refer to the same Assyrian king (i.e. Sargon).

⁽⁴⁶⁾ My analysis of 2 Kgs 17,3-6 clashes with the conclusions of H. WEIPPERT, "Die 'deuteronomistischen' Beurteilungen der Könige von Israel und Juda und das Problem der Redaktion der Königsbücher", *Bib* 53 (1972) 304-323, according to which the first edition of the DtrG was written in the time of Hezekiah and also included 2 Kgs 17,1-6. It is inconceivable that an author living at the time of the conquest of Samaria would have confused the operations of the king who had captured the city, deported its people and annexed it to Assyrian territory with those of his predecessor. Such confusion could have occurred only long after the conclusion of the events recorded in the passage.

⁽⁴⁷⁾ For an analysis of the work of the DtrH as an historian and antiquarian, see recently B. HALPERN, *The First Historians. The Hebrew Bible and History* (San Francisco 1988).

The mistaken dates in vv. 5-6 were the basis for a later redactor (possibly DtrN) who composed 2 Kgs 18,9-12⁽⁴⁸⁾. The passage is entirely dependent on 17,5-6 and is the result of the desire of the redactor to incorporate the fall of Samaria within the chronological framework of the reign of Hezekiah⁽⁴⁹⁾. The redactor removed all the obscurities involved in the understanding of the text of 17,5-6 and composed a clear and consistent chronology of the stages in the fall of Samaria. Needless to say, the passage in 18,9-12 is worthless for historical reconstruction; it contributes instead to our understanding of the way in which scribes in the post-exilic period worked with the old texts, trying to interpret them, smooth difficulties and fill gaps in the chain of events.

V. 24 of 2 Kgs 17 originally most probably came after v. 6⁽⁵⁰⁾. It described the deportation of people from Babylonia and the peripheral areas east of the Tigris by the "king of Assyria", i.e. Sargon II⁽⁵¹⁾. A later author, writing a polemical composition against the Samaritans, detached the verse from its original place and used it as an introduction for his work. Vv. 6.24 thus recounted the episode of the conquest of Samaria by Sargon and the following two-way deportation: from Samaria to northern Mesopotamia and Media and from Babylonia and its peripheral areas to Samaria.

Conclusion

Amalgamation of biblical and extra-biblical data always involves great difficulties arising from differences of outlook, diver-

⁽⁴⁸⁾ ŠANDA, *Könige*, 244-245; E. WÜRTHWEIN, *Die Bücher der Könige. I. Kön. 17-2. Kön. 25* (ATD 11,2; Göttingen 1984) 410.

⁽⁴⁹⁾ J. A. MONTGOMERY-H. S. GEHMAN, *A Critical and Exegetical Commentary on the Books of Kings* (ICC; Edinburgh 1951) 482.

⁽⁵⁰⁾ TREBOLLE, "La caída", 150-151; A. D. H. MAYES, *The Story of Israel between Settlement and Exile* (London 1983) 126.

⁽⁵¹⁾ N. NA'AMAN-R. ZADOK, "Sargon II's Deportations to Israel and Philistia (716-708 BC)", *JCS* 40 (1988) 44-46. For a different dating of the deportation of the Babylonians and Cuthians to Samaria, see H. WINCKLER, "Die samaritanischen Ansiedler", *Alttestamentliche Untersuchungen* (Leipzig 1892) 97-107; M. COGAN, *Imperialism and Religion. Assyria, Judah and Israel in the Eighth and Seventh Centuries B.C.E.* (Missoula 1974) 101, n. 23, with earlier literature; H. TADMOR, "On the History of Samaria in the Biblical Period", *Eretz Shomron* (The Thirtieth Archaeological Convention, September 1972) (Jerusalem 1973) 70-71 (Hebrew).

gence of literary genres and differences in the aims of the scribes. Attempts to combine them into a unified historical picture involves reconstructions and hypotheses, the best reconstruction is one that best fits the majority of evidence and is not contradicted by any unequivocal testimony. It is in this light that we must examine the problem at hand. No reconstruction fits all the documentary evidence — every proposed solution must include a detailed explanation for those sources which either contradict it or are at variance with it.

In the first part of this article we have critically examined the widely-accepted reconstruction according to which Samaria was conquered twice — once after three years of siege by Shalmaneser V (723 or 722 BC) and for the second time by Sargon II (720 BC). The reconstruction rests mainly on the chronological data of 2 Kgs 17,5b-6 and 18,9-10, according to which Samaria, after three years of siege, was captured by Shalmaneser in Hoshea's ninth year. Since Sargon explicitly claimed in his inscriptions that he had conquered Samaria, a two-conquest hypothesis was necessary to reconcile the conflicting sources. The hypothesis, however, involves rejection of various details that appear in Sargon's inscriptions and in the book of Kings.

As shown in the discussion, Sargon's inscriptions can hardly be reconciled with the two-conquest hypothesis. The prominent place of the conquest of Samaria in inscriptions composed in Sargon's later years, his statement that the rebellion of the Samaritans had already started in the time of his predecessor, the place of the Samaritans in the struggle of 720 BC and Sargon's claim that it was he who suppressed the rebellion and conquered Samaria — all these data do not accord well with the hypothesis that Samaria was conquered by Shalmaneser after a long siege. The archaeological excavations of Samaria hardly support a theory of a long siege that culminated with the conquest of the city. They can likewise be interpreted as evidence for a conquest after a short siege and limited destruction. The Babylonian Chronicle sets the "ravaging" of Šamara'in in Shalmaneser's accession year. Provided that the ravaged city is indeed Samaria (see note 27 above), the Chronicle may refer to a western campaign conducted by Shalmaneser at the beginning of his reign, a campaign that is also mentioned in 2 Kgs 17,3 and is probably referred to in the Tyrian history of Menander. The notation in the Babylonian Chronicle can hardly refer to a

conquest after a long siege in Shalmaneser's last year, as was suggested by the two-conquest hypothesis.

Similar problems arise out of the interpretation of the account in 2 Kgs 17,3-6. The text of v. 4b, according to which Hoshea was arrested before the siege of Samaria, contradicts the dates in 17,6a and 18,9-10. Dividing the operations related in v. 6 between two Assyrian kings, Shalmaneser (v. 6a) and Sargon (v. 6b), although only one "king of Assyria" is mentioned, is likewise problematic. Division of the account in vv. 3-6 into originally two parallel sources (vv. 3-4 and 5-6) is also not supported by analysis of the text.

As noted above, the chronological datum in v. 6a (and in 18,9-10) and the statement that Samaria was besieged for three years are the cornerstones of the two-conquest hypothesis. We suggest that the dates are not of archival source, but rather historical inferences based on interpretation of the source material by the Deuteronomistic Historian. Unfortunately his chronological interpretation of the sources was mistaken and a later redactor, trying to convert DtrH's dates into a chronological sequence of events, expanded the error into a scheme of mistaken dates (18,9-10). When removing the cornerstones of the two-conquest hypothesis, we are left with only one conquest of Samaria mentioned both in the Bible and in the Assyrian royal inscriptions.

Eliminating the erroneous dates enabled us to suggest a new reconstruction of the events leading to the fall of Samaria, and this reconstruction fits all the available data much better. The history of Hoshea as related in 17,3-4 accords well with the Assyrian, Babylonian and Tyrian sources. According to the description in the book of Kings, Hoshea clashed twice with the Assyrians, once in Shalmaneser's accession year (v. 3) and once towards the end of the latter's reign (v. 4). Hoshea's (and possibly Eloulaios') second act of rebellion was encouraged by the king of Egypt, apparently Pi(ankhy) of the 25th (Kushite) Dynasty.

When the Assyrian army (under Shalmaneser or his general) reached the Land of Israel, Hoshea appeared before him and was arrested (v. 4b). Shortly afterwards Shalmaneser died and it is not clear whether a siege of Samaria was under way during his last year. When Sargon ascended the throne, he was detained for a year and a half in Assyria, trying to curtail the inner rebellion and consolidating his rule. The Samaritans, whose king was held in prison (as a hostage?) were then able to join the coalition of Assyrian vassal

states and provinces crystallizing under the leadership of the king of Hamath and supported by Egypt. Only after Sargon's victories in the north and south did the city of Samaria come under a siege, one which apparently did not last long. Three years passed between Hoshea's imprisonment and the conquest of Samaria by Sargon, a period regarded by the Deuteronomistic Historian as years during which the city was under siege.

Sargon II is the king who conquered Samaria, annexed it to the Assyrian territory, deported its people and brought in others to take their place. His inscriptions give many accurate details about the history of Samaria in its last stage; the fact that the episode was artificially dated in the royal annals to the beginning of the king's reign⁽⁵²⁾ does not reduce the accuracy of the account. Conquest, annexation and deportation were stages in one and the same historical episode and should be assigned to Sargon's reconquest of the West in the year 720 BC. The Deuteronomistic Historian likewise described these stages in sequential order (2 Kgs 17,5-6.24); it is only his erroneous chronological data that marred the accuracy of his account of the final stage of the Northern Kingdom.

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SOMMAIRE

L'hypothèse répandue, selon laquelle Samarie aurait été conquise par Salmanasar V (723/722 av. JC) et par Sargon II (720 av. JC), n'est pas tenable. Le roi Osée a eu deux conflits avec les Assyriens: d'abord pendant le *rēš šarrūti* de Salmanasar (727 av. JC), ensuite vers la fin de son règne (724/722 av. JC). Il semble vraisemblable d'admettre qu'Osée ait été encouragé à cette deuxième tentative de rébellion par Pi(anky) de la 25ème dynastie. Sargon est le roi qui conquiert la Samarie (2 R 17,5-6a), qui déporta ses habitants (v. 6b) et la fit coloniser par d'autres (v. 24). Le soi-disant siège de trois ans (v. 5b) et sa date, fournie au v. 6a, sont à attribuer à des conclusions historiques fausses du DtrG.

(52) TADMOR, "Campaigns", 22-26, 30-36.

ANIMADVERSIONES

Implicazioni semantiche in alcuni casi di *Qere-Ketib*

Il fenomeno del *Qere-Ketib*⁽¹⁾ nella Bibbia ebraica pone dei problemi di interpretazione che restano ancora insoluti⁽²⁾. Gli interventi dei vari autori sulla questione sono solitamente intesi a chiarire l'origine del *Q-K*, la sua funzione, le sue caratteristiche. Le loro posizioni sono però spesso contrastanti e sembra mancare una trattazione organica che dica una parola risolutiva. In particolar modo, quattro autori hanno affrontato il problema negli ultimi decenni, aprendo anche nuove prospettive.

Nel 1937, Gordis pubblicava una monografia sull'argomento, poi riedita nel 1971 con un ampio Prolegomenon⁽³⁾. In essa l'autore, dopo aver presentato le varie teorie e ipotesi, precisa la propria e offre in appendice una dettagliata classificazione di tutti i *Q-K*, suddivisi in 85 gruppi. Per quel che riguarda l'origine e la natura del *Q-K*, egli distingue tre diversi stadi e livelli: quello in cui l'intervento del *Qere* serve da guida per il lettore a proposito del Tetragramma e di termini sconvenienti; quello in cui il *Qere* indica la pronuncia di termini e forme che, per l'assenza di vocali, potevano presentare problemi di lettura; infine quello in cui il *Qere* indica le varianti, rispetto al testo del *Ketib*, che si trovavano in altri manoscritti. Perciò, rispetto al testo scritto (*Ketib*), nei primi due stadi di sviluppo del *Q-K*, il *Qere* serviva ad indicare il modo di lettura di quello stesso testo, mentre nell'ultimo stadio serviva a segnalarne le varianti testuali più significative.

(1) Lungo il corso dell'articolo verrà indicato con l'abbreviazione *Q-K*.

(2) Per un'utile e sintetica esposizione del fenomeno del *Q-K*, cfr. A. DOTHAN, «Masorah», *EncJud* XVI (specialmente 1409-1410, 1419-1421) e l'ancor più specifico articolo di G. E. WEIL, «Qere-Kethibh», *IDBSup*, 716-723. Si veda inoltre C. D. GINSBURG, *Introduction to the Massoretico-Critical Edition of the Hebrew Bible* (London 1897) 183-186; I. YEIVIN, *Introduction to the Tiberian Masorah*, translated and edited by E. J. Revell (Masoretic Studies 5; Chico 1980) 49-64. Per interventi più puntuali su questioni particolari, cfr. A. RUBINSTEIN, «A Kethib-Qere Problem in the Light of the Isaiah Scroll», *JSS* 4 (1959) 127-133; I. YEIVIN, «The Vocalization of Qere-Kethiv», *Textus* 2 (1962) 146-149; A. SCHENKER, «Affranchissement d'une esclave selon Ex 21,7-11», *Bib* 69 (1988) 547-556; S. E. FASSBERG, «The origin of the *Ketib/Qere* in the Aramaic portions of Ezra and Daniel», *VT* 39 (1989) 1-12.

(3) R. GORDIS, *The Biblical Text in the Making: A Study of the Kethib-Qere* (Philadelphia 1937; New York 1971).

Orlinsky, nel 1940⁽⁴⁾, critica questa posizione e nel 1959 presenta la propria teoria⁽⁵⁾. Egli suppone che i Massoreti abbiano basato il loro lavoro di vocalizzazione su tre manoscritti da essi scelti come i più affidabili. Quando poi accadeva che tra quei tre manoscritti ci fossero delle divergenze, essi sceglievano la lettura attestata dalla maggioranza — e sarebbe il *Qere* —, e lasciavano come *Ketib*, non vocalizzato, la forma che compariva nel manoscritto rimanente⁽⁶⁾.

Sperber⁽⁷⁾ invece, spiega il *Q-K* come un modo con cui gli scribi, nello stabilire il testo di riferimento, preservavano ambedue le recensioni che egli ritiene siano alla base del testo biblico.

In tempi molto più recenti, la discussione è stata riaperta da Barr⁽⁸⁾ e arricchita di nuove prospettive. Egli sottopone a critica le teorie precedenti, offre una nuova classificazione dei *Q-K* (basata sulle differenze tra le due letture), e riimpone il problema proponendo un nuovo tipo di approccio. Egli stabilisce come punto di partenza la tradizione orale, di lettura del testo⁽⁹⁾, e su questo basa la propria interpretazione del *Q-K*. In questa prospettiva, il *Qere* sarebbe la lettura consolidata e autorevole che può aver preceduto la scelta e la determinazione di un testo scritto normativo e che, anche senza trovare riscontro in alcun manoscritto, viene indicata come il modo stabilito di lettura di quel testo⁽¹⁰⁾.

La discussione è tutt'ora aperta e non è nostra intenzione entrare nel merito, né, ancor meno, presentare un'ulteriore ipotesi. Il nostro intervento non intende riprendere la questione dell'origine del *Q-K*, né discutere della funzione ad esso intenzionalmente affidata dai Massoreti. Vorremmo piuttosto, partendo dal fatto della sua esistenza testuale, indicare delle possibilità semantiche che in alcuni casi si vengono a verificare per il fatto che il *Q-K* sia presente in un testo e così ci sia stato tramandato.

⁽⁴⁾ H. M. ORLINSKY, «Problems of Kethib-Qere», *JAOS* 60 (1940) 30-45. Cfr. anche, dello stesso autore, un intervento puntuale su un caso preciso di *Q-K*: «The Import of the Kethib-Qere and the Masoretic Note on לֵקְחָהּ», *JQR* 31 (1940-1941) 59-66.

⁽⁵⁾ H. M. ORLINSKY, «The Origin of the Kethib-Qere System: a New Approach», *VTS* 7 (1960) 184-192.

⁽⁶⁾ In appoggio a questa teoria, l'autore cita il famoso testo talmudico sui tre manoscritti trovati nel cortile del Tempio (*J Ta'anit* IV, 2 e passi paralleli in *Masechet Soferim* e *Abot de-Rabbi Nathan*: cfr. ORLINSKY, «The Origin», 189-191).

⁽⁷⁾ A. SPERBER, «Problems of the Masora», *HUCA* 17 (1942-1943) 299-315.

⁽⁸⁾ J. BARR, «A New Look at Kethib-Qere», *OTS* 21 (1981) 19-37.

⁽⁹⁾ La «reading tradition», in opposizione al tradizionale punto di partenza costituito dalla «manuscript tradition»: cfr. BARR, «A New Look», 35.

⁽¹⁰⁾ Nella nota finale del suo articolo, Barr apre un'ulteriore pista. Portando alle estreme conseguenze la sua posizione, egli giunge a suggerire, come ipotesi suggestiva, che il vero scopo del *Qere* fosse non la modificazione, ma la salvaguardia del testo scritto, cioè del *Ketib*. Il *Qere*, infatti, rappresentando il modo di lettura tradizionale, doveva essere conosciuto a memoria; il fatto che venisse precisato poteva perciò servire non tanto ad indicare come bisognava leggere, ma piuttosto a segnalare che il *Ketib* era corretto e doveva essere mantenuto com'era (BARR, «A New Look», 36-37, n. 31).

Il fenomeno della polisemia

È ormai riconosciuta, nel testo biblico, la presenza di espressioni a valenza polisemica che giocano a diversi livelli e che sono solitamente ricondotte all'autore stesso del testo. Soprattutto in tempi recenti, lo studio degli esegeti si è fatto attento a questo specifico fenomeno stilistico e si moltiplicano gli interventi tesi a sottolineare l'uso di vocaboli che, per la loro ricchezza semantica, producono particolari effetti di senso.

Ci riferiamo soprattutto alla polisemia riscontrata in termini che vengono usati appunto per sfruttare la loro ambiguità, creando particolari giochi verbali (spesso indicati col nome di «*double entendre*» o «*double entente*») di grande efficacia⁽¹⁾. Il doppio senso del termine può essere utilizzato in modo semplice all'interno della frase⁽²⁾, oppure secondo una prospettiva «bifronte», per cui uno dei due sensi della parola ambigua si riferisce a ciò che precede, e l'altro a ciò che segue⁽³⁾.

Ma può anche accadere che la polisemia (semplice o bifronte) sia determinata dalle diverse vocalizzazioni che si possono supporre o imporre

⁽¹⁾ Ciò che qui ci interessa non è tanto l'accorgimento retorico della ripetizione, nello stesso contesto, di termini omonimi, per cui si usa la stessa parola prima in un senso e poi in un altro: cfr. ad esempio G. R. DRIVER, «Linguistic and Textual Problems: Isaiah XL-LXVI», *JTS* 36 (1935) 404, 406; id., «Problems and solutions», *VT* 4 (1954) 242-243; A. GUILLAUME, «Paronomasia in the Old Testament», *JSS* 9 (1964) 286-290; D. R. BLUMENTHAL, «A play on words in the nineteenth chapter of Job», *VT* 16 (1966) 497-501; G. R. DRIVER, «Playing on Words», *Fourth World Congress of Jewish Studies*, Papers I (Jerusalem 1967) 121-129. Va inoltre ricordata l'opera di C. C. TORREY, *The Second Isaiah: A New Interpretation* (Edinburgh 1928) 199-202, 325-326, 463, frequentemente citata dagli studiosi della polisemia biblica, ma che noi purtroppo non abbiamo potuto consultare.

Noi siamo piuttosto interessati all'uso singolo di un termine per il quale si vogliono intendere contemporaneamente i suoi due sensi. È ciò che i retori arabi medioevali chiamavano *talḥin* o *tawrīya*. Cfr. R. GORDIS, *The Book of God and Man: A Study of Job* (Chicago 1965) 167: «In *talḥin*, the effect derives not from the sound but from the meaning. The poet uses a word which has two distinct denotations, one primary to the context and the other secondary but also appropriate. His choice of the particular word, rather than a synonym, is dictated by the desire to bring both meanings simultaneously to the consciousness of the reader, ...». Cfr. anche id., *The Book of Job: Commentary, New Translation and Special Studies* (New York 1978) 34, 80, 229-230 (*inter alia*); N. M. BRONZNIK, «Two Unrecognized Cases of *Talḥin*», *Studies in Judaica, Karaitica and Islamica*, Presented to Dr. Leon Nemoy (Ramat-Gan 1982) 39-45.

⁽²⁾ Cfr. gli esempi di *talḥin* presentati nelle opere della nota precedente; cfr. inoltre: GUILLAUME, «Paronomasia», 283-285; DRIVER, «Playing on Words», 121-122; J. J. GLÜCK, «Paronomasia in Biblical Literature», *Semitics* 1 (1970), soprattutto 53-60; G. RENDSBURG, «Double Polysemy in Genesis 49:6 and Job 3:6», *CBQ* 44 (1982) 48-51; B. J. SEGAL, «Double Meaning in the Song of Songs», *Dor le Dor* 16 (1987-1988) 249-255 (con bibliografia supplementare).

⁽³⁾ Cfr. C. H. GORDON, «New Directions», *BASP* 15 (1978) 59-60; G. RENDSBURG, «Janus Parallelism in Gen 49:26», *JBL* 99 (1980) 291-293; E. ZURRO, «Disemia de *brḥ* y paralelismo bifronte en Job 9,25», *Bib* 62 (1981) 546-547; D. GROSSBERG, «Pivotal Polysemy in Jeremiah xxv 10-11a», *VT* 36 (1986) 481-485. Facciamo notare che c'è una certa varietà di terminologia per indicare lo stesso fenomeno.

a uno stesso gruppo consonantico⁽¹⁴⁾ e che ne modificano il senso creando diverse possibilità di lettura. In questo caso, l'intervento massoretico, con la sua vocalizzazione, elimina l'ambiguità della forma consonantica, che è quella usata dall'autore⁽¹⁵⁾, indicandone una sola possibilità di lettura e di senso. Tale ambiguità resta invece intoccata quando la polisemia riguarda radici o parole omonime⁽¹⁶⁾.

Facciamo qualche esempio. Nel suo studio sul libro di Giobbe, Gordis, illustrando il fenomeno retorico del *talḥin*, presenta un caso molto interessante di polisemia⁽¹⁷⁾. In Giob 7,6, il sostantivo *tiqwā* ha il doppio e simultaneo significato di «speranza» (che è l'accezione normale del termine) e «filo» (come in Gios 2,18). Il senso del versetto sarebbe allora il seguente: «i miei giorni sono più veloci della spola del tessitore, finiscono senza speranza / per mancanza di filo». Il filo della vita si consuma, nel movimento veloce della spola, e con esso finisce la speranza dell'uomo.

In Cant 2,12, invece, la polisemia si presenta in modo bifronte⁽¹⁸⁾. Il termine *zāmīr* infatti può avere sia il significato di «potatura», sia quello di «musica, canto»: il primo in riferimento a ciò che precede («i fiori sono apparsi nei campi»), e il secondo in riferimento a ciò che segue («e la voce della tortora si ode nella nostra campagna»). Ambedue i sensi sono intesi contemporaneamente e dipingono artisticamente la primavera come tempo di fiori e di viti, di canti e di tubar di tortore nei campi.

Ma abbiamo anche detto che tale gioco semantico può basarsi sull'ambiguità della scrittura in quanto solo consonantica. Così, ad esempio, in Giob 3,6 il *talḥin* riconosciuto nel verbo *yhd*⁽¹⁹⁾ fa riferimento a due diverse radici: *hḏh*, «rallegrarsi, gioire» (vocalizzato *yihad*, come nel TM) e *yhd*, «aver parte, esser computato» (vocalizzato *yēhad*, come solitamente proposto: cfr. BHS). Si sovrappongono così, nella stessa forma consonantica, due sensi diversi simultaneamente intesi: la notte del suo concepimento, che Giobbe maledice, non deve essere computata nei giorni dell'anno ed in essi non deve potersi rallegrare (cfr. anche v. 7).

E ancora, in Ger 25,10-11a l'espressione *'wr nr* potrebbe avere il contemporaneo duplice significato di «luce della lampada» (*'ōr nēr*, secondo il

⁽¹⁴⁾ Cfr. in particolare GLÜCK, «Paronomasia», 55-56 (cfr. anche 61-66); GORDIS, *The Book of Job*, 34; G. RENDSBURG, «Janus Parallelism», 291-293; id., «Double Polysemy», 48-51; GROSSBERG, «Pivotal polysemy», 481-485.

⁽¹⁵⁾ Cfr. RENDSBURG, «Double Polysemy», 51: «... the original reader... did not have to choose between the two double meanings. Faced with a purely consonantal text, the reader was readily able to discern the double polysemy». Cfr. anche G. A. F. KNIGHT, *Deutero Isaiah. A Theological Commentary on Isaiah 40-55* (New York-Nashville 1965) 17; GLÜCK, «Paronomasia», 56.

⁽¹⁶⁾ Va segnalato anche un altro tipo di fenomeno polisemico su cui si è recentemente posta l'attenzione. Si tratta dell'uso di vocaboli che presentano un doppio significato a seconda della lingua di riferimento (ad es., ebraico e greco, ebraico e egiziano, ebraico e assiro), e che vengono usati nel testo biblico proprio giocando su questa loro ambiguità. Cfr. G. A. RENDSBURG, «Bilingual wordplay in the Bible», VT 38 (1988) 354-357 (con bibliografia).

⁽¹⁷⁾ GORDIS, *The Book of Job and Man*, 168; cfr. id., *The Book of Job*, 80.

⁽¹⁸⁾ Cfr. GORDON, «New Directions», 59-60.

⁽¹⁹⁾ Cfr. GORDIS, *The Book of Job*, 34.

TM) e di «terra coltivata» (*ʾūr nīr*)⁽²⁰⁾. Si avrebbe così una polisemia bifronte in cui il primo senso si riferisce a quanto descritto nel v. 10 (voce degli sposi e suono della macina), mentre il secondo viene proseguito nel v. 11 (il paese sarà ridotto a una rovina).

Alla luce di quanto detto fin qui, a noi sembra di poter vedere qualcosa di analogo in alcuni casi di *Q-K*. Vogliamo con ciò situarci al di là dell'intenzione dei Massoreti, d'altronde non ancora unanimemente decifrata, e partire invece dalla situazione di *fatto* che il *Q-K* crea.

Tale situazione si presenta come un particolare stato del testo in cui sono presenti *contemporaneamente* due possibilità di lettura⁽²¹⁾ e a volte anche di interpretazione. In alcuni casi, infatti, il *Q-K* non si limita a segnalare variazioni solo fonetiche o di grafia convenzionale, ma indica nel *Qere* un termine che, anche se solo leggermente modificato, proviene da un'altra radice o presenta una forma che ne cambia il senso⁽²²⁾. In questi casi, dunque, due possibilità interpretative diverse convivono di fatto nello stesso testo; per qualunque ragione il *Q-K* esista, il Testo Massoretico che possediamo e a cui facciamo riferimento chiede di prendere atto di questo strano fenomeno.

La domanda che allora si pone è se non esistano dei casi di *Q-K* in cui questa simultanea presenza di due letture possa permettere la decifrazione di un gioco semantico all'interno dello stesso testo, come avviene, *analogamente*

⁽²⁰⁾ Cfr. GROSSBERG, «Pivotal polysemy», 481-485.

⁽²¹⁾ Va qui ricordato quanto notato da Gordis, che elenca alcuni casi di «conflatio» esistenti nel Testo Massoretico che testimoniarebbero il tentativo, da parte degli scribi, di preservare delle varianti testuali inglobandole nel testo stesso. A volte questo riguardava una sola parola; si veniva allora a creare un termine anomalo, una specie di ibrido che combinava insieme due diverse forme (cfr. GORDIS, *The Biblical Text in the Making*, 41-43; cfr. anche S. TALMON, «Double Readings in the Massoretic Text, I», *Textus* 1 (1960) 144-184; id., «Aspects of the Textual Transmission of the Bible in the Light of Qumran Manuscripts», *Textus* 4 (1964) 95-132, soprattutto 124-125; J.G. JANZEN, «Double Readings in the Text of Jeremiah», *HTR* 60 (1967) 433-447; S. TALMON, «Some Aspects of the Text of the Hebrew Bible», *Armenian and Biblical Studies* (ed. M.E. STONE) (Jerusalem 1976) 56-57). In molti casi, però, il *Qere* interviene su queste forme anomale di singole parole, esplicitando una delle due letture soggiacenti (in una sorta di procedimento inverso rispetto a quello che noi ricerchiamo). Ma si dà anche il caso di forme lasciate nel loro stato ibrido o «doppio»: cfr. ad es. Ger 2,11 (citato da GORDIS, *The Biblical Text*, 42); Is 23,11 e 63,3 (citati da TALMON, «Aspects of the Textual Transmission», 124-125). Cfr. inoltre quanto affermato in W. GESENIUS, *Hebräische Grammatik* (Leipzig 281909) 78d a proposito delle *formae mixtae*: «...Noch andere sind entweder überhaupt falsche Lesarten oder stellen eine absichtliche Verschweisung zweier verschiedener Lesarten dar».

⁽²²⁾ BARR, «A New Look», 28 ss, classifica i vari casi di *Q-K* in cinque gruppi, basandosi proprio sulle differenze che vi si vengono a creare. Egli distingue perciò tra: 1) casi che registrano solo un cambio di «spelling convention»; 2) casi che presentano cambi di tipo morfologico o sintattico; 3) casi con differenze fonetiche ma non semantiche; 4) casi con differenze semantiche ma non fonetiche; 5) casi con differenze sia fonetiche che semantiche. Egli fa notare che tali cambiamenti sono, tranne poche eccezioni, materialmente quasi insignificanti. Si tratta per lo più di piccole variazioni grafiche, che solitamente riguardano una sola lettera, anche se possono produrre una differenza semantica talvolta notevole (cfr. *ibid.*, 25-27).

mente, nei casi di polisemia semplice o bifrante di cui abbiamo parlato, soprattutto quando essa è basata su una doppia vocalizzazione. A noi sembra che questo sia possibile, e vorremmo proporre qualche esempio di tale assunzione contemporanea del *Qere* e del *Ketib* ⁽²³⁾.

Qere-Ketib e polisemia ⁽²⁴⁾

Abbiamo già esaminato, in altra sede, il caso di Giud 7,21, che gioca sulla doppia ambiguità del verbo *rw'* (*Hi*) e della forma *Qal* (*Qere*) e *Hiphil* (*Ketib*) di *nws* ⁽²⁵⁾. Qualcosa di simile sembra soggiacere anche all'interpretazione data da Barbiero a proposito del *Q-K* di Prov 3,34 (*w^ela'ānāwīm / w^el'nyym*) ⁽²⁶⁾.

⁽²³⁾ Nella sua classificazione dei diversi tipi di *Q-K*, GORDIS, *The Biblical Text in the Making*, 155-156, elenca cinque casi (1 Sam 2,16; 1 Re 22,49; Ez 42,16; Prov 19,7; 1 Cron 11,20) in cui il *Qere* e il *Ketib* possono essere combinati insieme per risolvere le difficoltà del testo. Egli mantiene perciò sia il *Qere* che il *Ketib*, ma leggendoli in modo susseguente, ambedue presenti nel testo, ma come fossero scritti uno dopo l'altro. Non si tratta dunque di una assunzione simultanea e polisemica, ma vi si suppone comunque che non sempre e non necessariamente si debba operare una scelta a favore di una delle due forme. Cfr. pure nota 21. Anche nella tradizione giudaica antica e medioevale è presente la tendenza a spiegare e conservare ambedue le letture come ugualmente accettabili e pertinenti nel contesto. Nel suo studio sui casi di *Q-K* *lō' / lō*, Ognibeni parla di una «linea della duplice interpretazione», considerata tradizionale e molto diffusa tra gli esegeti ebrei medioevali, che cercava sempre di dare due spiegazioni, una per il *Qere* e l'altra per il *Ketib*. E riferendosi ai discepoli di Menahem ben Saruq, afferma che essi «sono partigiani della concezione secondo la quale ambedue, tanto il ketiv quanto il qere, hanno qualcosa da dire riguardo al senso del passo» (cfr. B. OGNIBENI, *Tradizioni orali di lettura e testo ebraico della Bibbia*. Studio dei diciassette ketiv *l' / qere lw* [Studia Friburgensia, Nouvelle Série 72; Fribourg 1989] 39). Nell'interpretazione del testo si poteva perciò tener conto sia del *Qere* che del *Ketib* e fare riferimento ad entrambi (cfr. *ibid.*, 37, 53, 54-55, 65, 69, 161, 179); questo a volte poteva essere fatto leggendoli, come fa Gordis nei cinque casi da lui proposti, in modo susseguente, cioè uno di seguito all'altro (si veda l'interpretazione di Menahem ben Saruq a proposito di Sal 139,16 in cui è presente il *Q-K* *l' (non) / lō* (per lui): «e non vi è per lui (ancora) un giorno solo dei giorni della sua vita»; *ibid.*, 199).

Ricordiamo infine il fenomeno della «*lectio duplex alternans*» segnalato, nel campo della paleografia greca, da P. PROULX-J. O'CALLAGHAN, «La lectura del salmo 88,21b (LXX) en 1 Clem 18,1», *Bib* 61 (1980) 92-101.

⁽²⁴⁾ Il testo ebraico cui facciamo riferimento per la discussione degli esempi è quello edito in *BHS* (Codex Leningradensis). Lo precisiamo perché non in tutti i manoscritti si trovano le medesime attestazioni di *Q-K*.

⁽²⁵⁾ Nel testo in questione, che si riferisce allo stratagemma di Gedeone nella guerra contro i Madianiti, l'ambiguità del verbo *rw'* e la presenza del *Q-K* sembrano quasi riprodurre la situazione di grande confusione conseguente al panico creato dall'attacco di Gedeone, in cui non si sa più chi è che fugge gridando spaventato e chi invece spaventa col suo grido e fa fuggire. Cfr. B. COSTACURTA, *La vita minacciata. Il tema della paura nella Bibbia Ebraica* (AnBib 119; Roma 1988) 107.

⁽²⁶⁾ Cfr. G. BARBIERO, «Il testo massoretico di Prov 3,34», *Bib* 63 (1982) 385: «... il termine conserva tutta la sua pregnanza, significata anche dal *Qere-Ketib* che i Massoreti hanno tramandato. In continuità con *yšrym* e *šdyqym*, ... anche qui, al concetto fondamentalmente sociologico di *nyym*, ... vediamo affiancarsi la connotazione morale di «umili, miti» a cui ci può portare il *Q* *nyym* e il parallelo con le circostanti qualificazioni». Possiamo integrare il discorso indicando anche Am 8,4; Sal 9,13.19; 10,12; Prov 14,21; 16,19, tutti testi che presentano la stessa polisemia indicata da Barbiero.

Presentiamo ora qualche altro esempio che serva ad illustrare il fenomeno in questione.

2 Sam 16,2b

...wayyô'mer šībā' haḥāmôrîm l'êbêt-hammelek lirkôb whlh̄m (Q: w^ehallelhem) w^ehaqqayîš le'êkôl hannê'ârîm w^ehayyayîn lištôt hayyâ'êp bammidbār.

Il testo fa parte del dialogo tra Davide e Ziba, che si svolge quando quest'ultimo raggiunge con doni il re fuggitivo durante la rivolta di Assalonne. Le offerte vengono elencate nel v.1: un paio di asini (*šemed ḥāmôrîm*), duecento pani (*mā'tayim lehem*), cento grappoli di uva passa (*mē'â šimmûqîm*), cento frutti estivi (*mē'â qayîš*)⁽²⁷⁾ e un otre di vino (*nēbel yāyîn*). Alla domanda di Davide sul senso di quei doni, il servo di Mephiboshet risponde con le parole del versetto che stiamo esaminando.

Egli nomina gli asini (*haḥāmôrîm*), i frutti (*w^ehaqqayîš*), e il vino (*w^ehayyayîn*): i primi da cavalcare (*lirkôb*), i secondi da mangiare (*le'êkôl*), il terzo da bere (*lištôt*). E c'è inoltre la parola del Q-K in questione: si tratta del pane, secondo il *Qere* (*w^ehallelhem*), ma sembra essere un verbo secondo il *Ketib*⁽²⁸⁾, che si riferisce agli asini e prosegue la precisazione di ciò a cui essi servono: *lirkôb ûl'ehillāhēm*⁽²⁹⁾.

La preferenza è generalmente accordata, da parte dei commentatori, al *Qere*, ed è questo che essi traducono, sia che indichino il problema testuale soggiacente⁽³⁰⁾, sia che non lo menzionino affatto⁽³¹⁾. Per quel che riguar-

⁽²⁷⁾ Sul termine *qayîš*, cfr. H. P. SMITH, *The Books of Samuel* (ICC; Edinburgh 1904) 347; S. R. DRIVER, *Notes on the Hebrew Text and the Topography of the Books of Samuel* (Oxford 1913) 318.

⁽²⁸⁾ Poiché il *Ketib* è formato di sole consonanti, la sua vocalizzazione può essere solamente ipotizzata e può accadere, come in questo caso, che non ci sia accordo tra gli autori. La vocalizzazione a cui noi facciamo riferimento è comunque quella che rende più facilmente ragione di tutte le consonanti presenti nel *Ketib*.

⁽²⁹⁾ Notiamo che nell'elencazione dei doni si precisano tre diversi destinatari: *bêt-hammelek* (la famiglia reale) per gli asini, *hannê'ârîm* (i servitori, ma anche i giovani e i soldati) per i frutti, e *hayyâ'êp bammidbār* (chi è sfinite nel deserto) per il vino. In senso più preciso, quindi, il pane (*Qere*) sarebbe per gli uomini di Davide, mentre il combattere (*Ketib*) farebbe riferimento alla casa reale. Non si può però insistere su questa suddivisione, perché si tratta ovviamente di doni globalmente offerti al re per la sua causa, e che devono servire insieme a lui e alla gente che lo segue.

⁽³⁰⁾ Cfr., *ad locum*, J. WELLHAUSEN, *Der Text der Bücher Samuelis* (Göttingen 1871); C. F. KEIL, *Die Bücher Samuels* (BC II 2; Leipzig 1875); K. BUDDE, *Die Bücher Samuel* (KHC VIII; Tübingen-Leipzig 1902); W. NOWACK, *Richter, Ruth und Bücher Samuelis* (HKAT I 4; Göttingen 1902); H. P. SMITH, *The Books of Samuel* (ICC; Edinburgh 1904); P. DHORME, *Les Livres de Samuel* (EB; Paris 1910); S. R. DRIVER, *Books of Samuel*; W. CASPARI, *Die Samuelbücher* (KAT VII; Leipzig 1926); G. BRESSAN, *Samuele* (La Sacra Bibbia; Torino 1954); R. DE VAUX, *Les livres de Samuel* (BJ; Paris 1961); J. MAUCLINE, *1 and 2 Samuel* (NCB; London 1971); G. BOCCALI, *I libri di Samuele* (NVB 8; Roma 1972); J. P. FOKKELMAN, *Narrative Art and Poetry in the Books of Samuel*, I: King David (Assen 1981); P. K. MCCARTER, *II Samuel* (AB 9; Garden City, NY 1984).

⁽³¹⁾ Cfr., *ad locum*, H. W. HERTZBERG, *Die Samuelbücher* (ATD 10; Göttingen 1960); L. ALONSO SCHÖKEL, *Samuel* (Los libros sagrados 4; Madrid 1973); P. R.

da poi la strana forma del *Ketib*, si tende a spiegare il primo *lamed* come in eccesso per una erronea ripetizione del *lamed* che inizia la parola precedente⁽³²⁾, e solo alcuni nominano il senso di «combattere», supponendo la vocalizzazione *ul^hhillāhēm*⁽³³⁾, che è quella a cui corrispondono le consonanti presenti nel testo⁽³⁴⁾.

In realtà, sia il *Qere* che il *Ketib* hanno il loro senso e sono entrambi accettabili e ben inseriti nel contesto del nostro racconto. Ziba viene infatti ad offrire al re un sostegno concreto che aiuti la sua fuga per mettersi in salvo, ma anche la sua lotta per riconquistare il potere⁽³⁵⁾. Perciò porta cibo (*hallehem*) per sostentare gli uomini di Davide, ma anche per consentire loro di combattere (*l^hhillāhēm*)⁽³⁶⁾. Le due letture non si escludono a vicenda, ma è soprattutto la loro «sovrapposizione» ad essere significativa⁽³⁷⁾. Nel-

ACKROYD, *The Second Book of Samuel* (CBC; Cambridge 1977); F. STOLZ, *Das erste und zweite Buch Samuel* (ZBKAT 9; Zürich 1981); R.P. GORDON, *1 and 2 Samuel: A Commentary* (Exeter 1986).

⁽³²⁾ Cfr. WELLHAUSEN, 199; S.R. DRIVER, 318; MCCARTER, 368. Invece FOKKELMAN, 456, preferisce pensare ad un *lamed* enfatico.

⁽³³⁾ Cfr. DHORME, 386 (citando Calmet); CASPARI, 610; DE VAUX, 207; BOCCALI, 386. Va ricordato che questi autori assumono sempre come unica accettabile la lettura indicata dal *Qere*. Solo Calmet sembra favorevole al *Ketib* e spiega la modificazione del *Qere* come determinata dal fatto di non considerare gli asini atti al combattimento (cfr. A. CALMET, *Commentaire littéral sur tous les livres de l'Ancien et du Nouveau Testament*, vol. II [Paris 1724] 567-568).

⁽³⁴⁾ Va qui segnalato che neppure tra gli studiosi del fenomeno del *Q-K* c'è accordo sulle vocali da assegnare al nostro *Ketib*. Così, mentre Cappellus precisa il significato «*ad proeliandum*», Gordis vocalizza «*ul^hhallehem*»; il primo elenca il nostro tra i casi di *lamed* in eccesso, il secondo tra quelli in cui il *Qere* è da preferire al *Ketib* (cfr. L. CAPPELLUS, *Critica Sacra* [Lutetiae Parisiorum 1650] 106; GORDIS, *The Biblical Text in the Making*, 152). Anche C.D. GINSBURG, *Prophetae Priores* (Londinii 1911) indica come *Ketib* la forma *ul^hhallehem*.

⁽³⁵⁾ La posizione del servo di Mephiboshet, a questo punto della narrazione, non è ancora chiara quanto a sincerità e solo in seguito dovrà confrontarsi con la versione dei fatti del suo padrone (cfr. 19,25-31). Sembra però esplicita la sua decisione di puntare su Davide come vincente; come afferma Fokkelman, «...his helping David involves a calculated risk: he expects that David will be the ultimate winner» (FOKKELMAN, *Narrative Art and Poetry*, I, 36).

⁽³⁶⁾ Come fa notare il Calmet, il fatto che degli asini siano menzionati per la guerra depista gli autori nell'interpretazione del *Ketib* (cfr. sopra, nota 33). In effetti, nei racconti biblici, sono solitamente i cavalli ad essere usati per combattere, ma non esclusivamente: così, proprio nella parte finale del nostro episodio, dove si narra la battaglia tra l'esercito di Davide e quello di Assalonne, si dice che quest'ultimo cavalcava un mulo (*pered*: 2 Sam 18,9; cfr. a tale proposito Y. YADIN, *The Art of Warfare in Biblical Lands*, 2 vols. [New York-Toronto-London 1963] 287). Nel III millennio, l'uso di carri da guerra trainati da asini o animali simili è attestato in Mesopotamia (ibid., 38; cfr. anche 128-129, 132-133) e in Egitto (cfr. G. CANSDALE, *Animals of Bible Lands* [Exeter 1970] 71). Inoltre, in 2 Re, gli asini (*hāmōrīm*) sono elencati con i cavalli tra i beni abbandonati dai soldati in fuga (cfr. 2 Re 7,7.10); anche se servivano solo per il trasporto di vettovaglie, essi facevano dunque parte dell'equipaggiamento di un esercito e trovavano una loro utilizzazione nel contesto bellico.

⁽³⁷⁾ Si potrebbe anche ipotizzare per questo testo una soluzione quale quella indicata da Gordis per altri casi di *Q-K* (cfr. sopra, nota 23), che mantiene ambedue le possibilità in modo susseguente. Ziba allora direbbe esplicitamente a Davide che gli porta gli asini per cavalcarli e per combattere, e il pane e i frutti per sfamarsi. Se però, come è nel Testo Massoretico, i due concetti di combattimento e pane vengono

le parole di Ziba, pane e guerra sono simultaneamente presenti e si confondono⁽³⁸⁾, in un'allusione reciproca giocata sul sottinteso. La prospettiva è così di combattere, ma sottilmente allusa, ambigualmente nascosta sotto il dono per sopravvivere⁽³⁹⁾.

Può essere qui interessante notare che, anche a livello narrativo, tutto l'atteggiamento di Davide durante la rivolta di Assalonne sembra muoversi sotto il segno di una certa ambiguità. Egli abbandona piangendo Gerusalemme, accettando gli eventi con addosso i segni dolorosi della penitenza e del lutto, ma nello stesso tempo invia Cusai ad ordire trame contro il figlio tanto amato e a sventarne i piani con l'inganno (cfr. 2 Sam 15,30-37). Perseguitato, accetta nella mitezza il volere di Dio fino a rispondere con il perdono agli insulti e alla maledizione di Simei (cfr. 2 Sam 16,5-14; 19,17-24), ma poi incarica Salomone di far morire quello stesso Simei a cui aveva anche promessa salva la vita (cfr. 1 Re 2,8-9). Mosso da un amore paterno che sa andare al di là dell'offesa subita, mentre prepara l'esercito allo scontro, invoca grazia per Assalonne e chiede ai suoi che ne abbiano riguardo. Ma accetta anche di non andare a combattere, e sarà proprio questa sua assenza dal campo di battaglia a rendere possibile l'assassinio del figlio (cfr. 2 Sam 18,1-15). Davide, prigioniero della propria regalità, uccide per vivere in obbedienza al proprio compito.

All'interno di questa intricata realtà si situa l'episodio di cui ci stiamo occupando. Ziba, servo ambiguo⁽⁴⁰⁾, offre a Davide il cammino dell'ambiguità. Le sue parole e i suoi doni invitano a vivere, ma sottintendono la necessità di uccidere, perché solo la distruzione dei suoi nemici può consentire

assunti come «sovrapposti», e non semplicemente «giustapposti», si avrebbe una lettura più sfumata, in cui l'ambiguità, basata sul detto e non-detto, gioca un ruolo importante, come diremo subito. Aggiungiamo che tale lettura simultanea dei due concetti si pone su una linea simile a quella della polisemia bifronte, in cui l'allusione del *Ketib* alla lotta — nascosta sotto la menzione del pane — fa riferimento a ciò che precede, mentre il *Qere* si rapporta a quanto segue.

⁽³⁸⁾ Può essere interessante qui ricordare la segnalazione di KÖNIG, *Stilistik, Rhetorik, Poetik* (Leipzig 1900) 10-12, citato da Finkel, a proposito del termine *le-hem* in Giud 7,13 che implicherebbe nel contesto il duplice significato di «pane» e «guerra» (per quest'ultima accezione, si veda Giud 5,8): cfr. J. FINKEL, «An Interpretation of an Ugaritic Viticultural Poem», *The Joshua Starr Memorial Volume* (Jewish Social Studies 5; New York 1953) 35 (con altri esempi di *double-entendre*).

⁽³⁹⁾ Serve qui ancora ripetere che non intendiamo interpretare l'esplicita intenzione che ha guidato i Massoreti nell'indicazione di questo *Q-K*, ma solo riferirci allo *stato attuale* del testo che possediamo. Detto questo, possiamo allora aggiungere che anche la menzione degli asini prosegue nella stessa linea di ambiguità. È possibile, come afferma Calmet, che la correzione del *Qere* rispecchi la difficoltà di associare gli asini all'idea della guerra (cfr. sopra, note 33 e 36). Ma proprio questo intervento massoretico, ora esistente nel testo, crea un nuovo senso del testo stesso. Ziba offre a Davide degli asini, apparentemente solo per il trasporto; invece il dono è un'esortazione a combattere e vincere. La *lectio facilior* del *Qere* non elimina, ma sottintende e spiega la *lectio difficilior* del *Ketib*.

⁽⁴⁰⁾ L'atteggiamento di Ziba nei confronti di Davide non è facilmente decifrabile, ma l'insieme della narrazione, il credito dato dal re alla spiegazione di Mephiboshet, e le ultime parole di quest'ultimo, sembrano alla fine smascherare la mala fede del servo: cfr. al proposito FOKKELMAN, *Narrative Art and Poetry*, I, in particolare 30-40. Così, la pretesa lealtà di Ziba verso Davide si coniuga con la slealtà nei confronti del suo diretto padrone: cfr. *ibid.*, 195.

al re di tornare a regnare. Così, attraverso la sottile allusione del testo, Davide assume la drammatica contraddizione e accetta la « guerra » come suo « pane ».

Precisato con questo testo il tipo di fenomeno a cui intendiamo riferirci, possiamo ora ad altri esempi che potremo esaminare più brevemente.

2 Sam 16,12

'ûlay yir'eh yhwh b'wny w'hēšib yhwh lî tôbâ taḥat qillâtô hayyôm hazzeh.

Siamo ancora nello stesso contesto del testo precedente, durante la fuga di Davide in seguito alla rivolta di Assalonne. Il re, giunto a Bacurim, viene maledetto da Simei ma si rifiuta di rispondere con la violenza a tale oltraggio. Il versetto di cui ci occupiamo è appunto la parte finale del dialogo tra Davide e i suoi seguaci, che vorrebbero invece reagire. Tutta la speranza di Davide è ormai nel Signore, che guarderà « *b'wny* »: *b'e'ênî* secondo il *Qere*, *b'a'wōnî*⁽⁴¹⁾ secondo il *Ketib*.

Il *Qere*, secondo la tradizione rabbinica, è un'emendazione di *b'e'ênô* / *b'e'ênaw*. Si tratterebbe cioè di un *Tiqqun Sopherim* operato per evitare un antropomorfismo (« suo occhio », cioè di Dio) che poteva sembrare eccessivo⁽⁴²⁾. Il Targum, seguito dalla maggior parte degli esegeti ebraici medioevali, interpreta nel senso di « mie lacrime, le lacrime del mio occhio »⁽⁴³⁾, in riferimento alla situazione di dolore che Davide sta vivendo.

Il *Ketib* può essere inteso o nel senso di « mia colpa, mia iniquità »⁽⁴⁴⁾, o in quello di « colpa commessa contro di me » (genitivo oggettivo)⁽⁴⁵⁾, o ancora in quello di « mia punizione »⁽⁴⁶⁾. In genere però gli autori preferi-

(41) Cfr. BH (Kittel); GORDIS, *The Biblical Text in the Making*, 148, e i seguenti commentatori (già citati per esteso nelle note 30 e 31 cui facciamo riferimento anche per le prossime note riguardanti 2 Sam): KEIL, 331; BUDDE, 276; SMITH, 349; DHORME, 387; S. R. DRIVER, 318; HERTZBERG, 284; DE VAUX, 209; FOKKELMAN, *Narrative Art and Poetry*, 456.

Diversa posizione assume il comitato presieduto da Barthélemy, che legge il *Ketib* secondo il senso espresso dalla LXX (ἐν τῇ ταπεινώσει μου), seguita da Vulgata e Siriaca (cfr. D. BARTHÉLEMY, *Critique Textuelle de l'Ancien Testament*, I [OBO 50,1; Fribourg 1982] 277); si suppone perciò per il *Ketib* la forma *b'e'onyî*.

(42) Cfr., con buona documentazione anche su altri aspetti riguardanti questo caso di *Q-K*, F. LANGLAMET, « David et la maison de Saûl », *RB* 86 (1979) 398-402; C. MCCARTHY, *The Tiqqune Sopherim and Other Theological Corrections in the Masoretic Text of the Old Testament* (OBO 36; Göttingen 1981) 81-85. Secondo Levin, non solo il *Qere*, ma anche il *Ketib* « appear to have been deliberate departures from *b'ynw* »: cfr. S. LEVIN, *The Father of Joshua/Jesus* (Binghamton, NY 1978) 73-74.

(43) Cfr. BARTHÉLEMY, *Critique Textuelle*, I, 277.

(44) Cfr. KEIL, 331; DHORME, 387; HERTZBERG, 284; DE VAUX, 209; ACKROYD, 150; MCCARTHY, *The Tiqqune Sopherim*, 82; MCCARTER, 369.

(45) Cfr. BUDDE, 276; SMITH, 349; S. R. DRIVER, 318; GORDIS, *The Biblical Text in the Making*, 195; MAUCLINE, 277.

(46) Cfr. FOKKELMAN, *Narrative Art and Poetry*, 456.

scono, con una piccola modificazione, leggere *b'e'onyî*, «mia miseria, afflizione, sofferenza», seguendo in ciò le antiche Versioni⁽⁴⁷⁾.

Ancora una volta, può forse essere possibile mantenere tutte le possibilità soggiacenti al testo, in una linea interpretativa che prosegue l'ambiguità di cui abbiamo già parlato sopra, a proposito del v. 2. Davide sta fuggendo, perseguitato e maledetto, e questo è certamente uno stato di afflizione e dolore (così, le Versioni) che si esprime nel pianto, cioè nelle «lacrime» del suo occhio (così il *Qere* nell'interpretazione giudaica). Ma tale miseranda situazione è consapevolmente vista e accettata da Davide come conseguenza della sua vicenda con Betsabea, come già annunciatogli da Natan (cfr. 2 Sam 12,10-12). Egli è vittima di una «iniquità commessa contro di lui» dal figlio Assalonne (che occasiona anche l'offesa di Simei), ma questo rivela la sua «colpa e iniquità», e ne rappresenta la «pena» (secondo le varie possibilità del *Ketib*). Davide fa riferimento al proprio dolore per appellarsi alla misericordia di Dio, ma ciò a cui Dio deve guardare è una sofferenza che rivela una colpa ormai confessata e profondamente sofferta e scontata⁽⁴⁸⁾. La speranza di Davide nel bene che il Signore potrà rendergli al posto della presente maledizione esprime così la fede di chi, riconoscendosi peccatore, ripone tutta la sua fiducia nel Dio che perdona.

Giud 19,3

wayyāqom 'išāh wayyēlek 'aḥārēhā l'dabbēr 'al libbāh lhšybw ...

Il testo si riferisce all'episodio del levita e della concubina di Betlemme che, ritornata dal padre, viene raggiunta dall'uomo per essere ricondotta a casa. Le espressioni usate sono *dibbēr 'al lēb* e l'infinito *Hiphil* di *šwb* con suffisso di 3ª persona maschile nel *Ketib* (*lahāšibō*) e di 3ª persona femminile nel *Qere* (*lahāšibāh*). In ogni caso, si tratta della descrizione dell'agire del levita che va a convincere la donna perché ritorni a casa. Nel *Qere*, però, l'esplicitazione è sul «far ritornare» la donna, mentre nel *Ketib* si tratta di «cambiare, convertire» il cuore della concubina⁽⁴⁹⁾. L'assunzione simultanea dei due aspetti rende più preciso il discorso: l'uomo non va semplice-

⁽⁴⁷⁾ Di tutti gli autori citati nelle precedenti note, si dicono favorevoli al *Ketib* solo Keil e Gordis, mentre Fokkelman lo indica come possibile.

⁽⁴⁸⁾ Ci sembra interessante a tale proposito il commento di Hertzberg al nostro testo. Egli traduce *b'wny* con «mein Elend», rimandando esplicitamente in nota a *BH*, ma poi, commentando, così scrive: «Bemerkenswert ist, dass in v. 12 das Wort "mein Elend" (*b'e'onji*) im *Ketib* als *ba'wōnī* = "meine Schuld" gelesen wird; danach hätte David nicht nur, wie nach dem vermutlich richtigen Text, sein jetziges Schicksal in Demut auf sich genommen, sondern auch die Berechtigung der göttlichen Vergeltung anerkannt» (HERTZBERG, 280, 284).

⁽⁴⁹⁾ Gli autori solitamente preferiscono tradurre il *Qere*, sia che indichino oppure no il problema del *Ketib*: cfr., *ad locum*, K. BUDE, *Das Buch der Richter* (KHAT VII; Freiburg-Leipzig-Tübingen 1897); W. NOWACK, *Richter - Ruth* (HKAT I, 4; Göttingen 1902); M.-J. LAGRANGE, *Le livre des Juges* (EB; Paris 1903); G.F. MOORE, *Judges* (ICC; Edinburgh 1908); A. VINCENT, *Le livre des Juges - Le livre de Ruth* (BJ; Paris 1958); H.W. HERTZBERG, *Die Bücher Josua, Richter, Ruth* (ATD 9; Göttingen 1973); R.G. BOLING, *Judges* (AB 6A; Garden City, NY 1975); J.D. MARTIN, *The Book of Judges* (CBC; Cambridge 1975); J.A. SOGGIN, *Judges: A Commentary* (OTL; London 1987).

mente a riprendere la compagna e riportarla indietro, ma va per convincerla, così che il suo ritornare da lui coincida con il «ritornare» del suo cuore. Si manifesta così la vera portata di quel gesto e il desiderio che lo muove: far tornare fisicamente a sé la donna non basta, ché il vero ritorno deve passare per il cambiamento del cuore.

Ger 18,22

tiššāma' z'e'āqā mibbāttēhem kī tābī' 'ālēhem g'e'dūd pit'ōm
kī kārū š/šyħh l'elokdēni ūpaħim tām'enū l'raglāy.

Siamo all'interno delle cosiddette «confessioni» di Geremia. Il profeta si lamenta e invoca l'intervento divino perché siano puniti i suoi nemici che tramano insidie per eliminarlo. Nella seconda parte del versetto in esame, egli descrive l'attività dei suoi avversari contro di lui, come già aveva fatto al v. 20, ove diceva: *kī kārū šūhā l'napšī*.

L'espressione nei due versetti è molto simile e viene generalmente interpretata nello stesso modo (se non addirittura indicata come doppiione)⁽⁵⁰⁾: i nemici hanno scavato una fossa per catturare Geremia; e in parallelo, al v. 22, si dice anche che gli hanno teso lacci ai piedi.

Nel v. 22 c'è però un problema di scrittura, evidenziato dal *Q-K*. Il *Qere* infatti legge *šūhā*, che è il termine usato al v. 20, mentre il *Ketib* sembra supporre il termine *šihā*. Il senso non cambia, e gli autori traducono comunque con «fossa, buca»; alcuni poi affermano di preferire il *Qere*⁽⁵¹⁾, altri invece indicano la forma del *Ketib* come semplice variante dell'altra⁽⁵²⁾.

In realtà, ambedue i termini si trovano usati come complemento oggetto del verbo *krh* in altri testi e con lo stesso senso: *šihā* in Sal 57,7 e 119,85; e *šūhā*, come già visto, in Ger 18,20. La LXX però in alcuni casi traduce diversamente e dà ai termini in questione senso di «parola, discorso»⁽⁵³⁾. Così, abbiamo ῥήματα in Ger 18,20; λόγον in Ger 18,22, e ἀδολεσχίας in

Il *Ketib* viene invece preferito da GORDIS, *The Biblical Text in the Making*, 139, 190, e da H. W. JÜNGLING, *Richter 19. Ein Plädoyer für das Königtum: Stilistische Analyse der Tendenzzerzählung Ri 19,1-30a; 21,25* (AnBib 84; Rome 1981) 96-97.

⁽⁵⁰⁾ L'ipotesi spesso sostenuta è che l'espressione del v. 20 sia una variante del v. 22, da cui dunque proviene: cfr. B. DUHM, *Das Buch Jeremia* (KHAT XI; Tübingen-Leipzig 1901) 158; W. RUDOLPH, *Jeremia* (HAT 12; Tübingen 1968) 122; R. P. CARROLL, *The Book of Jeremiah. A Commentary* (OTL; London 1986) 380; W. MCKANE, *Jeremiah*, vol. I (ICC; Edinburgh 1986) 440. Né Rudolph né McKane traducono il v. 20; lo stesso fa E. W. NICHOLSON, *The Book of the Prophet Jeremiah*, Chapters 1-25 (CBC; Cambridge 1973) 158. Usano invece accorgimenti tipografici (come i caratteri diversi o le parentesi), F. GIESEBRECHT, *Das Buch Jeremia* (HAT III, 2, 1; Göttingen 1907) 108; A. GELIN, *Jérémie* (BJ; Paris 1951) 105.

⁽⁵¹⁾ Cfr. GIESEBRECHT, *Jeremia*, 108; A. CONDAMIN, *Le livre de Jérémie* (EB; Paris 1920) 155.

⁽⁵²⁾ Cfr. K. H. GRAF, *Der Prophet Jeremia* (Leipzig 1862) 269; RUDOLPH, *Jeremia*, 122; W. L. HOLLADAY, *Jeremiah I: A Commentary on the Book of the Prophet Jeremiah*, Chapters 1-25 (Hermeneia; Philadelphia 1986) 527. Si veda anche CAPPELLUS, *Critica Sacra*, 153; GORDIS, *The Biblical Text in the Making*, 129.

⁽⁵³⁾ Per una più completa documentazione testuale, cfr. MCKANE, *Jeremiah*, I, 439-440.

Sal 119,85. Sembra perciò che sia da supporre, da parte della traduzione greca, una lettura *śihâ*, «conversazione, meditazione, discorso»⁽⁵⁴⁾. Ma questa può essere anche la forma *Ketib* del nostro testo di Ger 18,22⁽⁵⁵⁾, dal momento che nel *Ketib* manca la distinzione tra *š* e *ś*.

In questo caso, si potrebbe pensare a una sorta di gioco sui concetti di trappola fisica, fossa, e trappola verbale, complotto⁽⁵⁶⁾. Tentando la lettura simultanea del *Qere* e del *Ketib*, si verrebbe a creare un sottinteso che in qualche modo sovrappone metafora e realtà: i nemici scavano la fossa a Geremia (cfr., in parallelo, *ûpahîm tāmēnû*...), ma è una fossa fatta di parole, costruita sul complotto, gli intrighi e la diffamazione (cfr. v. 18: *wayyô'merû lekû wēnaḥšēbâ 'al yirmēyāhû maḥāšābôt*⁽⁵⁷⁾; v. 19: *qôl yēribāy*; v. 23: *kol 'āšātām 'alay lammāwet*).

Terminiamo questa piccola serie di esempi indicando brevissimamente altri tre casi:

Ger 2,3

qōdeš yiśrā'el lyhwh rē'sît tbw'th

Israele è la primizia del raccolto di Dio (*Q*: *tēbû'ātô*), ma è insieme primizia del suo proprio raccolto, offerta da donare a Dio in obbedienza alla sua legge (leggendo per il *K*: *tēbû'ātāh*, in riferimento a Israele, la sposa cui ci si rivolge, nel versetto precedente, al femminile).

Sal 100,3

dē'û kî yhwh hû' 'ēlōhîm hû' 'āsānû wl' 'ānaḥnû⁽⁵⁸⁾

L'essere stati fatti da Dio vuol dire riconoscere di non essere noi l'origine di noi stessi (*K*: *wēlô'*: «Egli ci ha fatti, e non noi»), e perciò di appartenere a Colui che ci ha creati (*Q*: *wēlô'*: «Egli ci ha fatti, e noi siamo suoi»).

Ger 1,5

bē'terem 'šwrk babbe'ten yēda'tikā...

Il *Qere* legge *'eššorkā*, da *yšr*, «formare», mentre il *Ketib* è generalmente preso come *'āšûrekā*, da una radice *šwr* con lo stesso senso. Si potrebbe anche pensare ad altro *šwr* con senso di «rinchiudere», in opposizione

⁽⁵⁴⁾ Cfr. CARROLL, *The Book of Jeremiah*, 380; HOLLADAY, *Jeremiah* 1, 527; MCKANE, *Jeremiah*, 439-440.

⁽⁵⁵⁾ Cfr. HOLLADAY, *Jeremiah* 1, 527 e MCKANE, *Jeremiah*, 439-440.

⁽⁵⁶⁾ Cfr. HOLLADAY, *Jeremiah* 1, 527, che così spiega l'apparente ripetizione di fraseologia nei vv. 20 e 22.

⁽⁵⁷⁾ Cfr. pure, nello stesso versetto, l'espressione *wēnakkēhû ballāšôn*: il progetto è di colpire il profeta nel suo parlare e per il suo parlare; ma la lingua (portatrice di minacce, false accuse, accordi omicidi) può essere anche il mezzo per colpire, una delle armi a cui i nemici ricorrono per annientare l'avversario.

⁽⁵⁸⁾ Per lo status quaestionis e l'analisi di questo caso di *Q-K*, cfr. OGNIBENI, *Tradizioni orali di lettura*, 60, 107-108, 190-193. Questo autore accorda la sua preferenza al *Qere*.

a *yš'* del secondo emistichio⁽⁵⁹⁾. Holladay invece, basandosi su un suggerimento di M. Dahood a proposito del Sal 77,3, ipotizza anche per Ger 1,5 un verbo *šwr* con senso di «chiamare, nominare»⁽⁶⁰⁾.

In riferimento a quest'ultima possibilità, se accettata, diventerebbe possibile vedere nella sovrapposizione grafica del *Q-K* una sovrapposizione anche tematica dei due concetti di «formare» (e eventualmente «rinchiudere») e «chiamare»: la formazione nel ventre da parte di Dio è già una chiamata; nel grembo materno, vocazione e dono di identità personale si identificano.

Conclusione

Concludendo, ci sembra che questi pochi esempi possano suggerire la possibilità di una lettura simultanea del *Qere* e del *Ketib* che rispecchi la situazione testuale creata dall'intervento massoretico e che insieme apra nuove piste interpretative. Il problema posto dal fenomeno preso in esame resta difficile e non conosce una soluzione univoca. Ma può diventare possibile, almeno in alcuni casi, la scoperta di una realtà polisemica, creata dalla presenza di fatto del *Q-K*, nella quale si rivelano nuove implicazioni e più articolati rapporti concettuali all'interno del testo biblico.

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⁽⁵⁹⁾ Cfr. G. DEL OLMO LETE, *La vocación del líder en el Antiguo Israel*. Morfología de los relatos bíblicos de vocación (Biblioteca Salmanticensis III Studia 2; Salamanca 1973) 267.

⁽⁶⁰⁾ Cfr. HOLLADAY, *Jeremiah* 1, 20.

Observations on Early Papyri of the Synoptic Gospels, especially on the "Scribal Habits"

Of the 96 papyri manuscripts of portions of the New Testament known to us from antiquity⁽¹⁾, John's gospel is represented in no less than 22 of these, Matthew's gospel in 18, Luke's gospel in 8 and Mark's gospel in 3⁽²⁾. This still-expanding resource is of decisive importance for New Testament textual criticism, providing evidence far earlier than the "Great Uncials" of Vaticanus, Sinaiticus, Alexandrinus and Bezae. But the information provided by these manuscripts goes far beyond the purely "text-critical", if by that we mean primarily the assessing of variant readings with a view to ascertaining the original text, and can contribute in many ways to our knowledge of early Christianity. Much can be discovered from a simple survey of the spread of material preserved.

If we select those papyri manuscripts of the gospels which can be dated to the fourth century or earlier we have 12 manuscripts with portions of John's gospel⁽³⁾; 12 manuscripts with portions of Matthew⁽⁴⁾; 4 manuscripts with portions of Luke⁽⁵⁾; and 2 manuscripts with portions of Mark⁽⁶⁾. This simple counting of numbers of manuscripts representing the different gospels is interesting. That the ratio of these numbers reflects the popularity of the respective gospels in the early church can be substantiated from other evidence, and particularly the evidence of the fathers⁽⁷⁾.

⁽¹⁾ This number is derived from K. and B. ALAND, *The Text of the New Testament* (Grand Rapids 1987) 96-101, where the list is taken to 88, and from the supplements listed in *Bericht der Hermann Kunst-Stiftung zur Förderung der neutestamentlichen Textforschung für die Jahre 1985 bis 1987* (Münster 1988) 59, 60, where the total is taken to 96. Full bibliographical information up to No. 95 is now available in J.K. ELLIOTT, *A Bibliography of Greek New Testament Manuscripts* (SNTSMS 62; Cambridge 1989). For information concerning content, dating etc. we have generally followed K. ALAND, ed., *Repertorium der griechischen christlichen Papyri. I Biblische Papyri: Altes Testament, Neues Testament, varia, Apokryphen* (Patristische Texte und Studien 18; Berlin 1976).

⁽²⁾ See the table in ALAND, *Text*, 85 and add P⁹⁰, P⁹³ and P⁹⁵ to John's count and P⁹⁶ to Matthew's count.

⁽³⁾ P⁵, 6, 22, 28, 39, 45, 52, 66, 75, 80, 90, 95.

⁽⁴⁾ P¹, 25, 35, 37, 45, 53, 62, 64 (= +67), 70, 71, 77, 86. P²¹ is listed in ALAND, *Text*, 97, as third century. Elsewhere, however, Aland gives a date of 4th to 5th cent (*Repertorium*, 241), since this later date is that assigned by the original editors as well (*Oxyrhynchus Papyri* 10 [1914] 14-16) we have not included this in our list here.

⁽⁵⁾ P⁴, 45, 69, 75.

⁽⁶⁾ P⁴⁵, 88.

⁽⁷⁾ At a simple level one could check the space devoted to citations from each of the gospels in the listings of *Biblica Patristica: Index des citations et allusions bibli-*

Interestingly, this order for the gospels: Matthew, John, Luke, Mark is the order in which the gospels are presented in the "Western" group of manuscripts (so D, some of the Old Latin MSS, Gothic etc.). On the other hand, for text critical purposes the information gained from the papyri containing John's gospel is far greater than those containing Matthew, due to the fact that several almost complete copies of John are extant (P^{45,66,75}), while the most that we have of Matthew in any one copy is a little over a chapter (P⁴⁵). In addition several Johannine fragments represent the earliest portions of the New Testament ever discovered (in particular P⁵², but now also P⁹⁰).

Perhaps the best example of the wider use of this evidence is the work of C.H. Roberts, who has assessed the importance of the papyrological evidence for the history of the codex⁽⁸⁾ and has used information about both the hardware (papyrus remains) and software (style of writing, nomina sacra etc.) of New Testament manuscripts to greatly illumine the early history of Christianity in Egypt⁽⁹⁾. Other examples could be mentioned, some focussing on the outward form of the manuscripts⁽¹⁰⁾, others on the content⁽¹¹⁾.

Another area in which profitable research has been done is in the determination of the habits and tendencies of the earliest scribes. It is this area that I would like to concentrate on in the remainder of this article. It

ques dans la littérature patristique (3 vols; Paris 1975, 1977, 1980, with a supplement on Philo, 1982). The first volume, which deals with the period up until Clement and Tertullian, has 70 pages for Matthew (223-293); 26 pages for Mark (293-319); 59 pages for Luke (319-378) and 36 pages for John. The second volume, which deals with the third century apart from Origen, has 64 pages for Matthew (235-299); 5 pages for Mark (299-304); 18 pages for Luke (304-322); and 31 pages for John (322-353). The figures for Origen (the third volume) are 57 pages for Matthew (224-281); 5 pages for Mark (281-286); 23 pages for Luke (286-309); and 38 pages for John (309-347). This would involve a ranking of the gospels in the early church in the order: Matthew, John, Luke, Mark, which fits exactly the ratio we have in early papyri manuscripts. On Matthew and John in particular see, for a discussion of the evidence, on John: F.-M. BRAUN, *Jean le Théologien et son Évangile dans l'Église ancienne* (Paris 1959) 69-296; and on Matthew: E. MASSAUX, *Influence de l'Évangile de saint Matthew sur la littérature chrétienne avant saint Irénée* (BETL 75; Louvain 1986; originally published in 1950).

⁽⁸⁾ C. H. ROBERTS – T. C. SKEAT, *The Birth of the Codex* (London 1985 reprint of 1983).

⁽⁹⁾ C. H. ROBERTS, *Manuscript, Society and Belief in Early Christian Egypt* (London 1979).

⁽¹⁰⁾ So, for example, E. A. JUDGE – S. R. PICKERING, "Biblical Papyri prior to Constantine: Some cultural implications of their physical form", *Prudentia* 10 (1978) 1-14; which is an attempt "to look at a few ways in which the outward characteristics of the papyri give expression to how early Christians were placed culturally in their times".

⁽¹¹⁾ So, for example, K. ALAND, "Bemerkungen zum Alter und zur Entstehung des Christogrammes anhand von Beobachtungen bei P⁶⁶ und P⁷⁵", *Studien zur Überlieferung des NT und seines Textes* (ANTT 2; Berlin 1967) 173-179; which in addition to recent studies in the *Nomina Sacra* (see J. O'CALLAGHAN, «*Nomina Sacra*» in *Papyris Graecis Saeculi III Neotestamentariis* [AnBib 46; Rome 1970]) shows that from the earliest period Christian scribes must have been responsible for the production of NT manuscripts.

was E. C. Colwell who pioneered the study of the "singular readings" of a manuscript with a view to ascertaining scribal habits⁽¹²⁾. A "singular reading" is a reading or variant which is unique to the manuscript under investigation (usually defined, pragmatically, as a reading found in neither Tischendorf nor NA²⁶ nor a particularly related MS). By isolating all the readings which are "singular" the discussion of scribal habits can be empiricised. In his study Colwell analysed the singular readings of P⁴⁵, P⁶⁶ and P⁷⁵, showing in particular the regular harmonisation to immediate context, and noting the individual characteristics of the several scribes⁽¹³⁾.

Colwell was followed by J. R. Royse, who assessed the singular readings of P⁴⁵, P⁶⁶, P⁴⁷, P⁶⁶, P⁷², P⁷⁵ — all the extensive, pre-300 AD texts of the New Testament⁽¹⁴⁾. He showed that many scholars make claims about scribal habits without justifying their generalisations⁽¹⁵⁾. Since "scribes make virtually any kind of error imaginable sometime or other"⁽¹⁶⁾, the discussion of scribal habits must go beyond stating theories, or showing examples, to isolate and evaluate the singular readings (which are assumed to be the creation of the scribe)⁽¹⁷⁾. Royse concluded that the commonest form of corruption was harmonisation, normally to the immediate context; stylistic and grammatical improvements and transpositions were also commonly found. One of the most consistent habits is omission: "the fact is that the six papyri studied here all demonstrate a tendency to shorten the text"⁽¹⁸⁾. This means that the old rule "prefer the shorter reading" should no longer dominate text-critical studies⁽¹⁹⁾.

The aim of this paper is to assess the evidence of the synoptic papyri in order to test the conclusions drawn by Royse. We shall leave aside the large manuscripts of the gospels (P⁴⁵, P⁷⁵) which were covered in Royse's thesis, and look to the fourteen smaller fragments of the gospels as a testing ground.

P¹ is a third-century text containing Matt 1,1-9.12.14-20⁽²⁰⁾. The preserved section is reasonably clear, with a page number (A) over the middle

⁽¹²⁾ E. C. COLWELL, "Scribal Habits in Early Papyri: A Study in the Corruption of the Text", *The Bible in Modern Scholarship* (ed. J. P. HYATT) (Nashville 1965) 370-389.

⁽¹³⁾ P⁶⁶ was undisciplined and sloppy, P⁴⁵ was concise, favoring brevity, P⁷⁵ favors clarity and style.

⁽¹⁴⁾ J. R. ROYSE, "Scribal Habits in the Transmission of New Testament Texts", *The Critical Study of Sacred Texts* (ed. W. D. O'FLAHERTY) (Berkeley Religious Studies Series; Berkeley 1979) 139-161; this is a summary of his *Scribal Habits in Early Greek New Testament Papyri* (ThD, Graduate Theological Union; Berkeley 1981).

⁽¹⁵⁾ An example would be the discussion of Matt 3,7.12 in B. M. METZGER, *A Textual Commentary on the Greek New Testament* (London-New York 1975) where in the first the personal pronoun is said to have been omitted by scribes, and in the second it is said to have been added by scribes.

⁽¹⁶⁾ ROYSE, *Scribal Habits in Early Greek New Testament Papyri*, 17.

⁽¹⁷⁾ So also COLWELL, "Scribal Habits", 108; and also HORT in the Introduction to *The New Testament in the Original Greek* (London 1896) 230-237.

⁽¹⁸⁾ ROYSE, *Scribal Habits in Early Greek New Testament Papyri*, 601f.

⁽¹⁹⁾ ROYSE, *Scribal Habits in Early Greek New Testament Papyri*, Chapter VIII. The Shorter Reading? (593-615).

⁽²⁰⁾ Published as POxy 2, in *Oxyrhynchus Papyri* 1 (1898) 4-7, see also ALAND, *Repertorium*, 215.

of the text. The text is similar in type to κ and B. Most of the variants are in the spelling of names in the genealogy, two of these appear to be singular readings: AMMINAΔAB (v. 4), and ABEIA⁽²¹⁾ (v. 7). The other singular readings are both omissions. Firstly in v. 16 where $\tau\omicron\nu$ is omitted from before $\iota\omega\sigma\eta\phi$, and in v. 17 where $\alpha\iota$ is omitted before $\gamma\epsilon\nu\epsilon\alpha\iota$ ⁽²²⁾. This manuscript has another interesting reading in v. 18 (recto line 12) where it has $\tau\omicron\Upsilon \Delta\epsilon \iota\Upsilon \chi\Upsilon$, a distinctly majority-text reading.

P⁴ has been variously dated from the third to the sixth century, Aland opts for a third-century date⁽²³⁾. It contains portions of Luke's gospel: 1,58f; 1,62-2,1; 2,6f; 3,8-4,2; 4,29-32; 5,3-8; 5,30-6,16⁽²⁴⁾. The text is described by Aland as "normal" and Merrell shows how close it stands to Vaticanus⁽²⁵⁾. The genealogy again provides several unusual spellings, some of these are singular readings⁽²⁶⁾: $\epsilon\sigma\lambda\alpha\iota$ (3,25); $\epsilon\lambda\mu\alpha\sigma\alpha\mu$ (3,28); and $\iota\omega\beta\eta\tau$ (3,32). This manuscript shares omissions with some other manuscripts⁽²⁷⁾, but in terms of singular readings they are mainly additions: $\tau\omicron\nu$ before $\kappa\upsilon\rho\iota\omicron\upsilon$ in 1,76; $\tau\omega$ before $\acute{\epsilon}\tau\epsilon\rho\omega$, and $\alpha\upsilon\tau\omicron\upsilon$ after $\delta\epsilon\zeta\iota\alpha$ in 6,6. Other singular readings are $\delta\epsilon$ for $\kappa\alpha\iota$ in 5,3; the word order in 1,64; $\chi\alpha\lambda\alpha\sigma\alpha\iota$ in 5,4; $\alpha\upsilon\tau\omicron\nu$ for $\alpha\upsilon\tau\omicron\upsilon\varsigma$ in 5,31; and $\rho\eta\gamma\nu\sigma\iota$ for $\rho\eta\zeta\epsilon\iota$ in 5,37. In 3,22 our manuscript reads $\kappa\alpha\tau\alpha\beta\eta\nu\alpha\iota \tau\omicron \pi\nu\epsilon\upsilon\mu\alpha \tau\omicron \acute{\alpha}\gamma\iota\omicron\nu \pi\nu\epsilon\upsilon\mu\alpha\tau\iota$ (regarded by Merrell as a simple copyist error). Notable in addition is the omission of the article before $\iota\eta\sigma\omicron\upsilon\varsigma$ in 5,31; 6,3,9; not singularly, but with the agreement of B almost alone among the manuscripts.

P²⁵ may have been written in the fourth century⁽²⁸⁾. This contains passages from Matthew 18,32-34; 19,1-3.5-7.9-10⁽²⁹⁾. The singular readings of this manuscript are: the order $\pi\omicron\nu\eta\rho\alpha\iota \delta\omicron\upsilon\lambda\alpha\iota$; the spelling of $\omicron\phi\iota\lambda\eta\nu$, and of $\epsilon\pi\iota$ (for $\epsilon\pi\epsilon\iota$) in 18,32; the use of $\omicron\upsilon\nu$ instead of $\kappa\alpha\iota \sigma\epsilon$ ⁽³⁰⁾, and the order of $\eta\lambda\epsilon\eta\sigma\alpha$ [$\sigma\epsilon$] in 18,33. The spelling of $\omicron\sigma\tau\alpha\iota$ in 19,6; the word-order of 19,9b; the omission of $\acute{\eta} \alpha\iota\tau\iota\alpha$ and the substitution of $\gamma\iota\nu\epsilon\tau\alpha\iota$ for $\epsilon\sigma\tau\iota\nu$ in 19,10.

⁽²¹⁾ According to S. C. E. LEGG, *Euangelium secundum Matthaeum. Novum Testamentum Graece secundum textum Westcotto-Hortianum* (Oxford 1940) this reading is also found in 183.

⁽²²⁾ This is not completely singular, according to Legg it is missing from D, and some versions.

⁽²³⁾ *Repertorium*, 215.

⁽²⁴⁾ See J. MERELL, "Nouveaux fragments du Papyrus 4" *RB* 47 (1938) 5-22 for a transcript and plates. He gives a fourth-century date (7).

⁽²⁵⁾ MERELL, "Nouveaux fragments", 7f.

⁽²⁶⁾ This time determined against the apparatus of The American and British Committees of the International Greek New Testament Project, *The Gospel According to St. Luke; Part One: Chapters 1-12; Part Two: Chapters 13-24* (The New Testament in Greek III; Oxford 1984, 1987). This appears to have missed the reading of P⁴ of $\mu\alpha\tau\theta\alpha\theta\iota\omicron\upsilon$ (3,26), which P⁴ shares with two lectionaries and cop^{bo}.

⁽²⁷⁾ In 1,64 $\tau\omicron$ is omitted before $\omicron\nu\omicron\mu\alpha$; in 1,68 $\kappa\upsilon\rho\iota\omicron\varsigma$ is omitted; in 3,9 $\kappa\alpha\lambda\omicron\nu$; in 5,39 $\kappa\alpha\iota$; in 6,3,4 $\omicron\nu\tau\epsilon\varsigma \acute{\omega}\varsigma$; in 6,15 $\tau\omicron\nu$ are all omitted.

⁽²⁸⁾ ALAND, *Text*, 97; *Repertorium*, 246.

⁽²⁹⁾ O. STEGMÜLLER, "Ein Bruchstück aus dem griechischen Diatessaron (P. 16388)", *ZNW* 37 (1958) 223-229.

⁽³⁰⁾ $\kappa\alpha\iota \sigma\epsilon$ is omitted in some Coptic and Syriac MSS, according to Legg.

P³⁵ is variously dated, but is probably from the third or fourth century⁽³¹⁾. It contains Matt 25,12-15.20-23⁽³²⁾. This agrees almost exactly with the text of NA²⁶; it has no singular reading⁽³³⁾.

P³⁷ is a third- or fourth-century MS⁽³⁴⁾. It is also one of the most extensive samples that we have of Matthew (containing Matt 26,19-52)⁽³⁵⁾. There are eleven singular readings: the addition of an extra ταυτα before τη νυκτι ταυτη in v.31; the addition of και before ο Ιησους in v.34; the addition of δε before ωδε in v.38; the spelling of εγρηγορειτε in vv.38,40 and 41; ισχυσαν for ισχυσατε in v.40; the omission of the εισ-prefix from εισελθητε in v.41; the omission of an entire phrase from vv.49 and 50: Χαῖρε, ῥαββί· καὶ κατεφίλησεν αὐτόν. ὁ δὲ Ἰησοῦς εἶπεν αὐτῷ⁽³⁶⁾. The final singular reading is the omission of των from v.51.

P⁵³ is a pair of third-century fragments, one of Matt 26,29-40 and the other Acts 9,33-10,1. It seems probable that the two originally came from a single work, perhaps a codex of the four gospels and Acts (like Bezae and P⁴⁵)⁽³⁷⁾. The text is close to the main early Alexandrian witnesses, and there appear to be only two singular readings: the order εγω εν σοι in 26,33, and the spelling τρεις in v.34.

P⁶² is a fourth-century codex of very small size (6.6 × 5.6 cm), perhaps originally used as an amulet (it contains the text of Matt 11,25-30 in Greek and Coptic, as well as Dan 3,51-53.55)⁽³⁸⁾. The text of Matthew has few peculiarities: a unique order of αποκριθεις ειπεν ο [Ιησους in 11,25; and two spelling variations: εγρυψας for εκρυψας in v.25, and κωπιωντες for κοπιωντες in v.28.

P⁶⁴ and P⁶⁷ were originally published separately⁽³⁹⁾ but were soon recognised as coming from the same codex of Matthew from around 200 AD⁽⁴⁰⁾. This is thus the earliest piece of Matthew's gospel, with parts of Matt 3,9.15; 5,20-22.25-28; 26,7-8.10.14-15.22-23.31-33. In text-type this

⁽³¹⁾ See ALAND, *Repertorium*, 257.

⁽³²⁾ For the text see E. PISTELLI in *Papiri greci e latini della Società Italiana* 1 (1912) 1-2.

⁽³³⁾ The omission of the third ταλαντα in 25,22 is found in several MSS and versions, according to Legg.

⁽³⁴⁾ See ALAND, *Repertorium*, 37.

⁽³⁵⁾ See H. A. SANDERS, "An Early Papyrus Fragment of the Gospel of Matthew in the Michigan Collection", *HTR* 19 (1926) 215-226.

⁽³⁶⁾ This is almost certainly an accidental omission due to homoioteleuton with ειπεν, as SANDERS, "An Early Papyrus Fragment", 218.

⁽³⁷⁾ See H. A. SANDERS, "A Third Century Papyrus of Matthew and Acts" *Quantulacumque: Studies Presented to Kirsopp Lake* (eds. R. P. CASEY et al.) (London 1937) 151-161.

⁽³⁸⁾ L. AMUNDSEN, "Christian Papyri from the Oslo Collection", *Symbolae Osloenses* 24 (1945) 121-147.

⁽³⁹⁾ R. ROCA-PUIG, "P. Barc. Inv. N.1 (Mt. III,9,15; V,20-22, 25-28)", in *Studi in onore di Aristide Calderini e Roberta Paribeni* (3 vols; Milan 1956, 1957, 1956) II, 87-96. C. H. ROBERTS, "An Early Papyrus of the First Gospel", *HTR* 46 (1953) 233-237.

⁽⁴⁰⁾ See R. ROCA-PUIG, "Nueva publicación del papiro numero uno de Barcelona", *Helmantica* 37 (1961) 5-20; and C. H. ROBERTS, "Complementary note to the article of Prof. Roca-Puig", *Helmantica* 37 (1961) 21f.

MS appears quite close to the modern “Neutral” text, but in terms of the number of words extant, we don’t have too much to go on. It differs from NA²⁶ for example only in the omission of αὐτην in 5,28 (with others), and in the omission of εἰς ἕκαστος in 26,22 (with others). The only singular reading appears to be the spelling γαλεγλαιαν for Γαλιλαιαν in 26,32.

P⁶⁹ is a single leaf of a third-century codex, containing Luke 22,41.45-48.58-61⁽⁴¹⁾. It presents a rather “idiosyncratic” text. Luke 22,42-44 is absent (along with the first phrase of v.45). The most plausible explanation for this is that vv.43 and 44 were absent from the exemplar, and the scribe’s eye moved on from προσηυχeto in v.41 to προσευχης in v.45. The editors calculated that these would have been on the end of lines, four lines apart⁽⁴²⁾. In other words it is an accidental omission. In v.45 we have καθευδοντας κοι-[μωμενους, a singular reading which appears to be a case of harmonisation to the synoptic parallels (Matt 26,40 and Mark 14,37 both have καθευδοντας); this also involves the omission of αυτους. In v.47 we appear to have ἐφίλησεν τον Ιησουν, a Western reading. In 22,61 there is another singular reading: και στραφεις ο Πετρος, but the reason for this is entirely obscure.

P⁷⁰ is a third- (or possibly fourth-) century manuscript preserved in three fragments (and in two different museums). Portions of Matt 2,13-16; 2,22-3,1; 11,26-27; 12,4-5; 24,3-6.12-15 survive⁽⁴³⁾. Singular readings are as follows: the spelling of Ναζα[ρα in 2,23; in 11,27 this manuscript has ου]δεις γινωσκει τον υιον rather than επιγινωσκει (this is not quite singular); at 12,5 we read σαββατω, presumably this corresponds to εν τω σαββατω; this appears to be a singular reading (εν τοις σαββασιν is found in numerous witnesses). In 24,5 we find the reading εν τ[ο ονοματι μου instead of επι; in 24,6 two of the verbs are altered to infinitives: μελλεσ[ε]ται... θροε[σ]θαι. We also find a unique word-order in 24,14: το ευα[γ]ελιον τουτο της βασιλειας, and following these words ολην την οικoi[μενην, implying a preposition εις rather than εν. In this manuscript we have found several unique readings, but none involve either omission or addition of words.

P⁷¹ is a fourth-century fragment of Matthew 19,10-11.17-18⁽⁴⁴⁾. In this manuscript we find an omission of the article η before αιτια in v.10; and in v.18 Ιησους is omitted; these are unique readings.

P⁷⁷ is a very early fragment from a second-century codex of Matthew, containing Matt 23,30-39⁽⁴⁵⁾. The singular readings are two unusual cases of spelling in v.37: αποκτιν[ουσα and η]θεληκα. Otherwise the text stands closest to Sinaiticus, from which it varies on several points.

⁽⁴¹⁾ Published as POxy 2383 in *Oxyrhynchus Papyri* 24 (1957) 1-4.

⁽⁴²⁾ See *Oxyrhynchus Papyri* 24 (1957) 3.

⁽⁴³⁾ For Matt 11,26f and 12,4f see *Oxyrhynchus Papyri* 24 (1957) 4f; and for the rest see M. NALDINI, “Nuovi frammenti del vangelo di Matteo”, *Prometheus* 1 (1975) 195-200.

⁽⁴⁴⁾ *Oxyrhynchus Papyri* 24 (1957) 5f.

⁽⁴⁵⁾ *Oxyrhynchus Papyri* 34 (1968) 1-4.

P⁸⁶ is a fourth-century codex fragment of Matt 5,13-16.22.25. The text basically agrees with κ and B⁽⁴⁶⁾. Of interest is the reading $\mu\alpha\rho\alpha\nu\theta\eta$ in v. 13 (a singular reading for Matthew, but one found in a few miniscules at the parallel in Luke 14,34); the other singular readings: $\sigma\upsilon\delta\epsilon\nu\iota$ $\epsilon\iota\sigma\chi\upsilon\epsilon\iota$ in v. 13; $\tau\omicron$ $\mu\omicron\delta\iota\omicron\nu$ in v. 15; and $\pi\rho\omicron\sigma\}\phi\epsilon\rho\eta$ in v. 23 all appear to be spelling lapses on the part of the scribe.

P⁸⁸ is a leaf from a fourth-century codex, containing Mark 2.1-26⁽⁴⁷⁾. There are many spelling variants⁽⁴⁸⁾; the singular readings are: $\tau\omega$ $\pi\nu(\epsilon\upsilon\mu\alpha\tau)\iota$ $\alpha\upsilon\tau\omega$ for $\tau\omega$ $\pi\nu\epsilon\upsilon\mu\alpha\tau\iota$ $\alpha\upsilon\tau\omicron\upsilon$ and $\sigma\tau\iota$ for $\tau\iota$ in v. 8; the order $\epsilon\gamma\epsilon\iota\rho\epsilon$ $\sigma\omicron\iota$ $\lambda\epsilon\gamma\omega$ in v. 11; the omission of the whole phrase $\omicron\iota$ $\delta\epsilon$ $\sigma\omicron\iota$ $\mu\alpha\theta\eta\tau\alpha\iota$ $\sigma\upsilon$ $\nu\eta\sigma\tau\epsilon\upsilon\omicron\upsilon\sigma\iota\nu$ in v. 18 (probably due to homoioteleuton).

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From this survey of the early papyrus manuscripts of the Synoptic Gospels we can conclude the following concerning the habits of early scribes: the greatest number of singular readings are in the area of spelling, particularly in place-names and personal names. If we total the other types of alterations we find we have had twelve different omissions, seven different additions (all in two MSS: P⁴ and P³⁷), and ten cases of transposition of words into a new order. We also suggested two cases where a text was harmonised to a synoptic parallel⁽⁴⁹⁾, and one in which it was harmonised to the immediate context.

In none of the texts have we had enough material to draw firm conclusions. Inasmuch as the aim was to test the conclusions made by Royse (who worked on a far larger corpus), then our results fully support his conclusions: scribes often harmonised texts (to the immediate context and to canonical parallels)⁽⁵⁰⁾, and they frequently transposed words into a new order. Most fundamental is the support given to the conclusion that omission is more common than addition.

In the past one of the fundamental principles of textual criticism has been the rule *lectio brevior potior*. This rule — “prefer the shorter reading” — goes back at least as far as Wettstein in 1730 who formulated the rule (*inter alia*) that “Of two readings, the fuller and more ample is not at once

⁽⁴⁶⁾ C. CHARALAMBAKIS — D. HAGEDORN — D. KAIMAKIS — L. THÜNGEN, “Vier literarische Papyri der Kölner Sammlung”, *ZPE* 14 (1974) 37-40.

⁽⁴⁷⁾ S. DARIS, “Papiri letterari dell’Università Cattolica di Milano: No. 6 Marco, Vangelo 2,1-26”, *Aegyptus* 52 (1972) 80-88.

⁽⁴⁸⁾ See the list in DARIS, “Papiri letterari”, 86-88.

⁽⁴⁹⁾ It is important to allow for the fact that almost all the manuscripts tested were of Matthew. In one of the Lucan manuscripts we found evidence that would suggest harmonisation to Matthew (P⁶⁹), and one case of a Matthean text possibly being harmonised to a Lucan parallel (P⁸⁶). The popularity of Matthew in the early period suggests that Luke and Mark would much more readily be harmonised to Matthew than *vice-versa*. Indeed this is found even at a later period (see the evidence for harmonisation to Matthew in Codex W of Mark in L. W. HURTADO, *Text-Critical Methodology and the Pre-Caesarean Text: Codex W in the Gospel of Mark* [SD 43; Grand Rapids 1981] 70f.).

⁽⁵⁰⁾ See B. H. STREETER, *The Four Gospels A Study of Origins* (London 41930) 139-144.

to be accepted, but rather the former”⁽⁵¹⁾. Wettstein was followed by Griesbach, who elevated this rule to number one in his discussion⁽⁵²⁾. Since then the principle has continued to be used in most text-critical studies⁽⁵³⁾. While earlier scholars such as A.C. Clark⁽⁵⁴⁾ and B.H. Streeter⁽⁵⁵⁾ challenged the consensus unsuccessfully, it is the work of Colwell and Royse which offers a conclusive answer to the question “Were early scribes more likely to add or omit material?”

We have shown, in support of Royse’s thesis, that in fact omission is the more common scribal habit. If early scribes were more likely to omit words and phrases from their texts (for whatever reasons) it follows that we should not prefer the shorter reading, but rather prefer the longer reading (other factors being equal)⁽⁵⁶⁾.

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⁽⁵¹⁾ “Inter duas variantes lectiones non protinus amplior atque prolixior breviori est praeferenda, sed contra potius”, J.J. WETTSTEIN, “Animadversiones et cautiones ad examen Variarum Lectionum N.T. necessariae”, *H KAINH ΔΙΑΘΗΚΗ Novum Testamentum Graecum* (2 vols; Amsterdam 1751,1752) II, 862. This essay is a reprint in a slightly different form of his *Prolegomena ad Novi Testamenti graeci editionem accuratissimam*... (Amsterdam 1730) 165-201. For ET and information C.L. HULBERT-POWELL, *John James Wettstein, 1693-1754: An Account of his Life, Work, and some of his Contemporaries* (London n.d. = 1938?) 117.

⁽⁵²⁾ “Brevior lectio, nisi testium vetustorum et gravium auctoritate penitus destituatur, praeferenda est verbosiori”, J.J. GRIESBACH, “Prolegomena”, *Novum Testamentum Graece* (London 21796) I, LX. See ET and discussion in B.M. METZGER, *The Text of the New Testament* (New York-Oxford 21968) 120. Elsewhere he writes that the principle *brevior lectio potior* “has been accepted as generally valid by both classical and Biblical scholars” (161).

⁽⁵³⁾ So HORT, *Introduction*, vol. 2.175f.

⁽⁵⁴⁾ *The Primitive Text of the Gospels and Acts* (Oxford 1914).

⁽⁵⁵⁾ *The Four Gospels*, 131-139.

⁽⁵⁶⁾ The evidence of P⁴ and P³⁷ (in which addition is very common) does, however, offer a caution — scribal additions have been shown to occur, although with less frequency than omissions. In the final analysis, of course, each variant must be assessed on its own merits.

Colossians 2,14: Metaphor of Forgiveness

ἐξαλείψας τὸ καθ' ἡμῶν χειρόγραφον τοῖς δόγμασιν ὃ ἦν ὑπεναντίον ἡμῖν καὶ αὐτὸ ἤρκεν ἐκ τοῦ μέσου πληρώσας αὐτὸ τῷ σταυρῷ.

Blotting out the adverse bond of debt (with our subscription to the regulations) which was presented against us, he removed it completely, nailing it to the cross.

This verse is concerned with the forgiveness of sins, expressed in terms of the cancellation of a bond. It presents a powerful metaphor for the effects of the death of Christ. So much is clear, but problems arise in trying to explain the origin of the metaphor and the grammatical difficulties of the verse.

According to the lexicons⁽¹⁾ a χειρόγραφον is a document, 'a (hand-written) document', 'a certificate of indebtedness' or 'a bond'. The word is found in the LXX only in Tob 5,3 and 9,5⁽²⁾. In secular literature it is found much more frequently, especially in commercial and legal documents⁽³⁾. From a study of cheirographs in the papyri E. C. Best⁽⁴⁾ concludes that a cheirograph is a private document in subjective form. In turning to Colossians he suggests that this essential characteristic of a cheirograph should be kept in view. The interpretation of Col 2,14 and the use of 'cheirograph' in the Fathers is almost entirely dependent on their exposition of our verse⁽⁵⁾. The varying conclusions to which they come only serve to illustrate the difficulty of arriving at a satisfactory interpretation.

I. Previous Studies

In the New Testament the word χειρόγραφον is found only here in Colossians 2,14⁽⁶⁾. The metaphor of the cancellation of the cheirograph has been interpreted in six different ways.

(1) W. F. ARNDT-F. W. GINGRICH, *A Greek-English Lexicon of the New Testament and Other Early Christian Literature* (Cambridge 1959) 886; H. G. LIDDELL-R. SCOTT, *A Greek-English Lexicon* (Oxford 1940) 1986.

(2) 'And he gave him the certificate in indebtedness'.

(3) J. H. MOULTON-G. MILLIGAN, *The Vocabulary of the Greek Testament* (London 1926) 687. For parallels in other literature see A. DEISSMANN, *Light From the Ancient East* (London rev. edn 1927) 332-334.

(4) E. C. BEST, *An Historical Study of the Exegesis of Colossians 2,14* (Rome 1956) 7.

(5) BEST, *Exegesis of Colossians 2,14*, 11-137; G. W. H. LAMPE, *A Patristic Greek Lexicon* (Oxford 1961) 1522.

(6) It is suggested, however, that the pledge made by Paul in Philemon 19 has all the marks of such a promissory note. The introduction of his own name, together with the twofold emphatic ἐγὼ, gives the statement the character of a formal and binding signature in which Paul undertakes to make good the compensation to Philemon.

1. *The Law of Moses*

The author of Ephesians is the earliest interpreter of Colossians. Writing a generation later, he takes up the language and theology of Colossians and reinterprets it for a church which has moved in the direction of early Catholicism and the sub-apostolic age. Eph 2,15 offers an interpretive paraphrase of Col 2,14. There is no parallel to the dramatic metaphor of the cheirograph nailed to the cross. The original setting of Colossians has been lost, so Ephesians takes up the phrase τοῖς δόγμασιν and applies it to the law of Moses, set out in legal ordinances. In a later generation, when the fire of the Jew-Gentile controversy had died down, it would be quite innocuous to speak of a great reconciliation of Jew and Gentile, of a breaking down of the dividing wall of hostility, and of an abolishing of 'the law of commandments and ordinances'. This cannot be what Col 2,14 means. The dative in Ephesians is governed by the preposition ἐν, which further facilitates an interpretation in terms of the Mosaic law itself, with its built-in propensity to legalism, rather than the obligation to obey the law. In Ephesians the result of this shift is the more general reconciliation of humanity, rather than the specific forgiveness of sins and the triumph of the cross which we observe in Colossians. Thus the Ephesian version looks like a misunderstanding, or at least a rephrasing of what we have in Colossians⁽⁷⁾.

The interpretation of Col 2,14 in terms of Eph 2,15, and of the cheirograph as the Mosaic law, is taken up by the Fathers of the School of Antioch⁽⁸⁾. Reacting against the misuse of allegory by the earlier members of the School of Alexandria, they attempted to determine the literal sense of scripture. Despite some differences in detail all members of this school held that the cheirograph could be interpreted with reference to the Mosaic law, and all explained the decrees in an instrumental sense showing how the cheirograph was blotted out. Theodore of Mopsuestia is the most outspoken exponent of this view. He considered the law to be a bond in so far as it held mankind in bondage and from attaining salvation. He understood the decrees to express the way in which the law was removed. Here he makes use of Eph 2,15, being the first of the Antiochene School to do so explicitly. The law of commandments was put aside by (ἐν) decrees. The facts and hopes of the Gospel will eventually supersede the ordinances of the law⁽⁹⁾. Because of his particular theological emphasis Theodore could not hold that the cheirograph also meant a debt consequent upon Adam's sin, as Chrysostom had done before him, and Theodoret after him. But the point is clear that all members of this school at least admitted the possibility that the cheirograph could refer to the law of Moses, and the decrees the means by which the removal of the cheirograph was effected.

⁽⁷⁾ So C.F.D. MOULE, *The Epistles to the Colossians and to Philemon* (Cambridge 1957) 98.

⁽⁸⁾ BEST, *Exegesis of Colossians 2,14*, 58-96.

⁽⁹⁾ H.B. SWETE, *Theodori Episcopi Mopsuesteni in Epistolas B Pauli*, Vol. I: *Introduction: Galatians - Colossians* (Cambridge 1880) 150-151.

Following the lead given by the Fathers of the School of Antioch a number of modern commentators have proceeded to interpret the cheirograph in the light of Eph 2,15 as the law of Moses⁽¹⁰⁾. G. Kittel⁽¹¹⁾ suggests that τοῖς δόγμασιν refers to the Mosaic law and its demands as the content of the cheirograph and its hostility to mankind. As Christ is elsewhere described as the end of the law, so we are told here that the δόγματα are blotted out by him and that δογματίζεσθαι is brought to an end. R. Leivestad⁽¹²⁾ is more subtle in his approach. Although he also understands τοῖς δόγμασιν in the light of Eph 2,15 as referring to the Mosaic law, he claims that a total identification of the cheirograph with the law is not necessarily implied. Instead it represents the debt we have incurred for ourselves through our trespasses against the law. The demands of the law form the basis of the charge against us. Thus not only the bond, but the law itself is against us. Col 2,14 is a dramatic way of expressing the cancelling both of our debt and also of the law itself. Leivestad claims that the abrogation of the law is a well-known Pauline idea, and that it is only the metaphorical mode of expression which is singular.

Can the cheirograph really be a reference to the Old Testament law? It seems hard to imagine how Paul or a Paulinist, even in a violent metaphor, could have spoken of the law in such drastic terms as 'tossed aside and nailed to the cross'. Although νόμος is found more than a hundred times in Paul, it does not occur at all in Colossians, and δόγματα is never used of the law in the New Testament. The essential characteristic of a cheirograph is that it is an autograph, written by one's own hand to authenticate an agreement. This can hardly be applied to the law.

2. *A Pact with Satan*

Irenaeus interpreted the cheirograph in terms of Christ's triumph over sin and its consequences without reference to Satan. It was left to Origen to do this. His theology was so all-comprehensive that he drew together the total spectrum of views available at the time. So, although he too understood the cheirograph to mean a bond resulting from personal sin, in his attempt to explain its origin and effects, he went on to suggest that this bond also resulted from man's contract with the devil⁽¹³⁾. Since the devil ruled over sin and death, the devil also holds the bond of man's sin. Origen went on to say that the devil retained this document of obligation, but drew back from suggesting that the cancellation of the bond involved paying a

⁽¹⁰⁾ T.K. ABBOTT, *The Epistles to the Ephesians and to the Colossians* (Edinburgh 1897) 254-255, who says that the figure must not be pressed too far; A.S. PEAKE, "The Epistle to the Colossians", *The Expositor's Greek Testament* (London 1903) 521; F. PRAT, *The Theology of St. Paul*, Vol. II (London 1927) 228, n. 1; J.L. BAGGOTT, *A New Approach to Colossians* (London 1961) 87, 97; F.F. BRUCE, "Colossian Problems Part 4: Christ as Conqueror and Redeemer", *BSac* 141 (1984) 296.

⁽¹¹⁾ G. KITTEL, *TDNT*, II, 231.

⁽¹²⁾ R. LEIVESTAD, *Christ the Conqueror: Ideas of Conflict and Victory in the New Testament* (London 1954) 102.

⁽¹³⁾ Origen, *Selecta in Psalmos* 38.2.5; *In Exodum* 6,9; *Comment. in Epist. ad Rom.* 5,3 and 5,9.

ransom or price to the devil. He simply maintained that the bond was cancelled when affixed to the cross. 'When Origen seems most in danger of being enmeshed by the lines of his highly personal explanation of the cheirograph, he frees himself simply by returning to the Scriptural words' ⁽¹⁴⁾. Although other Fathers mention Satan in their exposition of Col 2,14, none of them expound this view as comprehensively or as whole-heartedly as Origen.

The interpretation of the cheirograph as a covenant between Adam and the devil was revived by G. Megas ⁽¹⁵⁾ in 1928. He drew extensively on literature which presented this point of view, from the Fathers to modern times, explaining the nailing to the cross from magical rites, and the cheirograph as an IOU written out by Adam to the devil. However all the literature cited is much too late to have had any influence on the author of Colossians.

E. Lohmeyer ⁽¹⁶⁾ pursues a similar line of interpretation. Since the indebtedness of the cheirograph cannot be properly used of the relationship between God and man, Lohmeyer suggests that the only alternative is an agreement with the devil. Such an IOU was given by Adam to the devil in paradise at the time of the fall of man, in which he committed himself to a life of sin and death in exchange for certain benefits which the devil bestowed upon him. Lohmeyer attempts to support his interpretation by an emendation to the punctuation of the passage. He places a full-stop after τοῖς δόγμασιν and begins a new sentence with, 'what was hostile to us, that he removed, that he has nailed to the cross'. All this is most improbable. The word ὁ refers to the cheirograph and cannot be reconstructed as the beginning of a new sentence ⁽¹⁷⁾. Indeed the passage, like the epistle as a whole, neither mentions the devil nor a pact that man has concluded with him. For this reason this interpretation, which found only scant support in the Fathers, has not been taken up by any recent commentator.

3. *An IOU from Mankind to God*

The currently most popular interpretation is that the cheirograph is an IOU from mankind to God. The image is taken from ancient legal and commercial practice. There are many references to such bonds of debt and obligation in the papyri from the second century BC onwards ⁽¹⁸⁾. In Col 2,14 the cheirograph is a certificate of debt, created by man's sin and therefore autographed by him. This interpretation is attested in the Fathers. Irenaeus, who supplies the link between St. Paul and later exegesis, was the first to explain the cheirograph to mean the removal of sin and its

⁽¹⁴⁾ BEST, *Exegesis of Colossians 2,14*, 30.

⁽¹⁵⁾ G. MEGAS, "Das χειρόγραφον Adams: Ein Beitrag zu Col 2,13-15", *ZNW* 27 (1928) 305-310.

⁽¹⁶⁾ E. LOHMEYER, *Die Briefe an die Philipper, an die Kolosser und an Philemon* (Göttingen ¹³1964) 116-117.

⁽¹⁷⁾ See LEIVESTAD, *Christ the Conqueror*, 100 for fuller criticism.

⁽¹⁸⁾ MOULTON-MILLIGAN, *The Vocabulary of the Greek Testament*, 687. Cf. Tob 5,3; 9,5; Philemon 19.

consequences⁽¹⁹⁾. He took the subject of the verse to be Christ, and in this he was followed by all Patristic writers. According to Irenaeus the bond of man's debt resulted from Adam's sin. By this he meant the guilt and debt of punishment passed on to all men from Adam. He avoided the danger of interpreting this bond of sin in reference to the devil, and made it clear that man is obligated to God. His general line of interpretation was followed by Hippolytus, Tertullian, Origen and Jerome, all of whom understood the cheirograph to mean a bond resultant from personal sin.

In modern times this interpretation of the cheirograph as a bond of obligation was taken up by J. B. Lightfoot⁽²⁰⁾, according to whom the bond in question in Col 2,14 is signed by men's consciences. The Jewish people might be said to have signed the contract when they bound themselves by a curse to observe all the enactments of the Mosaic law⁽²¹⁾. But the second person plural seems to include Gentiles as well as Jews, so that a wider reference must be given to the expression to include the Gentile's recognition of obligation to what he knows of the will of God. The cheirograph is against us because we have manifestly failed to discharge its obligation⁽²²⁾.

The dative τοῖς δόγμασιν is problematic. There is much to be said for the omission of the line τοῖς δόγμασιν ὃ ἦν ὑπεναντίον ἡμῖν⁽²³⁾, since it disturbs the parallelism and reads like an inadequate attempt to explain χειρόγραφον. But there is no textual support for this, and interpretation by way of emendations to the text is unsatisfactory. Following J. B. Lightfoot⁽²⁴⁾, it is usually taken as depending on an implied γεγραμμένον, 'consisting of, or written in terms of decree'. J. A. T. Robinson⁽²⁵⁾ has made the suggestion that the dative might be due to the meaning of obligation in the cheirograph, and that the bond was an undertaking to keep the decrees. It therefore placed us under an obligation to them, like an IOU signed by our own conscience, so that what Paul is saying is now erased is our subscription to the ordinances. He further takes καθ' ἡμῶν as simply 'in our name', and holds that the cheirograph has stood in our name since Exodus 24,3 and is now against us to prove our guilt. It is admitted that this interpretation does attempt to solve the otherwise pointless repetition in ὑπεναντίον ἡμῖν, but the proposed use of κατά with the genitive is not very widely attested outside the context of swearing oaths. F. F. Bruce⁽²⁶⁾ feels that more force should be given to the dative

⁽¹⁹⁾ Irenaeus, *Contra Haereses* 5.17.3.

⁽²⁰⁾ J. B. LIGHTFOOT, *St. Paul's Epistles to the Colossians and to Philemon* (London 1876) 197.

⁽²¹⁾ Deut 27,14-26.

⁽²²⁾ Cf. Rom 7,16.22.23.

⁽²³⁾ As R. P. MARTIN, "Reconciliation and Forgiveness in the Letter to the Colossians", *Reconciliation and Hope: New Testament Essays on Atonement and Eschatology presented to L. L. Morris* (ed. R. BANKS) (Exeter 1974) 116-120, but he has to admit that there is no textual support for this.

⁽²⁴⁾ LIGHTFOOT, *Colossians and Philemon*, 187-188.

⁽²⁵⁾ J. A. T. ROBINSON, *The Body* (London 1952) 43, n. 1.

⁽²⁶⁾ F. F. BRUCE, *The Epistles to the Colossians, to Philemon and to the Ephesians* (NICNT; Grand Rapids 1984) 106, n. 18.

τοῖς δόγμασιν attaching to χειρόγραφον, and translates it as a dative of accompaniment, 'the bond, ordinances and all'. Alternatively E. Percy⁽²⁷⁾ takes the dative causally, linking τοῖς δόγμασιν with the following relative clause, ὃ ἦν ὑπεναντίον ἡμῖν, to provide the reason why the handwriting was against us. But this does too much violence to the word-order⁽²⁸⁾. However, Percy is at pains to stress the metaphorical nature of the cheirograph, and warns against forcing the metaphor by suggesting a document written by man himself and deposited with God. He thinks the verse presents a figurative paraphrase for the accusation of the law on account of man's transgression.

The interpretation of the cheirograph in terms of an IOU from mankind to God is acceptable as far as it goes, but seems to lack a specific application to the Colossian situation.

4. The Heavenly Book

It has been claimed that there is a Jewish-Christian exegesis of Col 2,14 behind Ode Sol 23,5-9, Gos. Truth 19,17 and Rev 5,1-5, according to which the cheirograph is interpreted in terms of a heavenly book⁽²⁹⁾. There is evidence, drawn from the Old Testament and Jewish literature, to show that the idea of such a book of words, kept by God and recalling all men's sins, was a familiar theme⁽³⁰⁾. It is also claimed that the actual term 'cheirograph' is used of this book in an anonymous apocalyptic writing from the first century BC⁽³¹⁾. In this text the accusing angel held in his hand a book in which was recorded the sins of the seer. He asked that they might be wiped out. There was also another book containing his good deeds. The καθ' ἡμῶν of Col 2,14 would designate the former. The idea is developed by Blanchette and Bandstra.

⁽²⁷⁾ E. PERCY, *Die Probleme der Kolosser und Epheserbriefe* (Lund 1946) 88-89.

⁽²⁸⁾ C. F. D. MOULE, *An Idiom Book of New Testament Greek* (Cambridge 1959) 45, n. 2.

⁽²⁹⁾ J. DANIÉLOU, *The Development of Christian Doctrine Before the Council of Nicaea*, Vol. I: *The Theology of Jewish Christianity* (London 1964) 192-204; O. BLANCHETTE, "Does the Cheirograph of Col 2,14 Represent Christ Himself?", *CBQ* 23 (1961) 306-312; A. J. BANDSTRA, *The Law and the Elements of the World* (Kampen 1964) 158-161; H. WEISS, "The Law in the Epistle to the Colossians", *CBQ* 34 (1972) 294-314; R. P. MARTIN, *Colossians and Philemon* (London 1973) 84-85.

⁽³⁰⁾ G. WIDENGREN, *The Ascension of the Apostle and the Heavenly Book* (Uppsala 1950); L. KOEP, *Das himmlische Buch in Antike und Christentum* (Bonn 1952) 55-56.

⁽³¹⁾ BANDSTRA, *The Law and the Elements*, refers to the "Apocalypse of Elijah", G. STEINDORF (ed.), *Die Apokalypse des Elias, ein unbekannte Apokalypse und Bruchstücke der Sophonias-Apokalypse* (TU 17; Leipzig 1899) 18-19. However no direct reference to the heavenly book, let alone to a cheirograph, can be detected in the translation of the text provided by O. S. Wintermute in *The Old Testament Pseudepigrapha*, Vol. I: *Apocalyptic Literature and Testaments* (ed. J. H. CHARLESWORTH) (London 1983) 735-753. Wintermute, 730, indicates that this is a composite work containing both Jewish and Christian materials dated between AD 150-275, and probably nearer to the later date. The late dating and uncertain reference to a heavenly book makes the evidence presented by Bandstra suspect.

According to Blanchette Col 2,14 is part of a brief aside on soteriology in which Paul applied the metaphor of the celestial book to Christ. The cheirograph represents not only the record of man's sins, but also the sinful body of flesh which Christ took upon himself at the incarnation. This body of flesh was nailed to the cross by the Father (the Father is taken as the subject of the sentence), as the sign that our sinful debt has been fully remitted. Τοῖς δόγμασιν is made to refer to the evangelical ordinances of the Father. In support of this view Blanchette draws attention to the link with 2 Cor 5,21 'Christ was made sin' and Gal 3,13 'Christ became a curse', and suggests that Paul is saying a similar thing in Col 2,14 in identifying Christ as 'the cheirograph against us'⁽³²⁾. However, it by no means follows from these two passages that in Col 2,14 the cheirograph is identified with Christ. In a study of them both M. D. Hooker finds no reason even to mention Col 2,14⁽³³⁾. Furthermore Blanchette tries to make the cheirograph mean too many things at once to be convincing, as he makes the transition from the heavenly book, to the body of flesh, and then to Christ himself.

Bandstra agreed that there was an early Jewish-Christian interpretation of Col 2,14 which identified the cheirograph with Christ himself. He follows Blanchette in identifying it with the body of flesh which Christ took upon himself, and draws the same parallel with 2 Cor 5,21 and Gal 3,13, but cannot take τοῖς δόγμασιν as the evangelical ordinances by which the blotting out was accomplished⁽³⁴⁾. Instead he believes that the cheirograph is presented by the denizens of the spirit world, and that the victory over the principalities and powers in Col 2,15 is closely associated with the robbing of Satan of his role of accuser. Understood in this way the argument in Col 2,14 would be that Christ took away from the accusing angel the instrument by which he carried out his work. But again to achieve this interpretation the cheirograph is made to mean first the heavenly book of accusation listing men's sins, then the sinful flesh of mankind, and finally Christ on the cross. In view of the widespread use of χειρόγραφον as a signed certificate of indebtedness this transition of thought is difficult to accept.

The interpretation of the cheirograph as a heavenly book indicting men's sins is based on Daniélou's suggestion that in the Odes of Solomon and in the Gospel of Truth we have an early Jewish-Christian exegesis of Col 2,14. Our own examination of these texts has shown that they come from a stage further down the road to Gnosticism than the situation as we see it at Colossae⁽³⁵⁾. On this basis we would question their usefulness in determining the meaning of Col 2,14.

⁽³²⁾ See B. LINDARS, *New Testament Apologetic* (London 1961) 234.

⁽³³⁾ M. D. HOOKER, "Interchange in Christ", *JTS* NS 22 (1971) 349-361.

⁽³⁴⁾ These are taken to be the law of Moses.

⁽³⁵⁾ R. YATES, "Colossians and Gnosis", *JSNT* 27 (1986) 49-68, especially 59-63 'Gnostic exegesis of Col 2,14'.

5. Penitential Stelae

W. Carr⁽³⁶⁾ draws attention to the penitential stelae found in Asia Minor in the second and third centuries AD. These preserve inscriptions which, he claims, fulfil the requirements of a cheirograph. They contain a personal autograph acknowledging indebtedness, and leading to condemnation if the required terms are not fulfilled. The stelae have five main characteristics:

1. They are all cultic. Those who have offended have failed in their religious duties, and in penitence set themselves apart for a period of time to perform religious functions.

2. They use as a key concept ἁμαρτία, a failure of duty or some sin of impurity.

3. This failure is the substance of the confession. The emphasis is on publicity for the crime.

4. The offended deity has demonstrated acceptance of this confession of guilt. This is evident in the sinner's glad acceptance of his punishment as a sign of the divine forgiveness.

5. Finally, the stele is set up as a record of events, and as a warning to others who may be tempted to sin.

Although the earliest example comes from AD 126, it is claimed that such public confessions were part of the religious life of Asia Minor in the first century, and would not be unknown to Paul or the Colossians. Could the background for the cheirograph be found in a local religious practice?

Τοῖς δόγμασιν are made to refer to the 'detail of personal decisions' which led to condemnation. The divine act corresponding to this act of man's autograph is found in that forgiveness which is related wholly to the cross. Carr explains that the crucifixion is the most serious crime of which man is guilty; the final line of man's cheirograph; his penitential stele on which are listed all his crimes. It is his ultimate decision, his δόγμα, which became the decree of Christ's death. But the cross, which was the ultimate line in man's listed confession, is paradoxically the way of destruction for that indictment of guilt.

One of the problems with Carr's argument is that the application of the word χειρόγραφον to the stelae is itself metaphorical, as compared with what we have in Col 2,14, where the metaphor of forgiveness includes the mental image of that which is written on paper. But the greatest flaw in Carr's proposition is that of dating. We cannot be sure that the Colossians knew of the kind of public confession and penitential stelae referred to, although his attempt to find a meaning for the cheirograph from the religious background of the Colossians is to be commended.

⁽³⁶⁾ W. CARR, "Two Notes on Colossians", *JTS* NS 24 (1973) 492-500; id., *The Background, Meaning and Development of the Pauline Phrase HAI ARCHAI KAI HAI EXOUSIAI* (Cambridge 1981) 55-56, 189, n. 33.

6. *Theophany Visions*

In the Fathers we find a bewildering variety of interpretations of Col 2,14 and of the cheirograph. This is reflected in the opinions of modern commentators. Eph 2,15 presents the earliest interpretation of Col 2,14, but does not actually use the word cheirograph, and takes τοῖς δόγμασιν as a reference to the law of Moses. Other early interpretations in terms of a secret book giving access to the divine realms are found in gnostic literature. But the Fathers do not take up this idea, probably because of its suspect links with Gnosticism. Even Eph 2,15 seems to be making a correction of what could be easily misunderstood in a gnostic direction.

Why is there no reference in the Apostolic Fathers to the cheirograph or to Col 2,14? Why does Eph 2,15 make such a significant shift in meaning? Why do the Fathers not take up the interpretation of the cheirograph in terms of the heavenly book? The answer to these questions may lie in the fact that a later generation saw fit to correct the abuses that the terminology of Col 2,14, and the interpretation of the cheirograph in a context of theophany visions, could lead to. Of all the Fathers we found that Irenaeus presents the most plausible interpretation of the verse in terms of a metaphor for the cancellation of sin. He is followed by many modern commentators, who take the cheirograph to be a bond of obligation similar to an IOU. However, it is our contention that such a unique metaphor needs to be rooted more clearly in the Colossian situation, so that it would make immediate sense to the recipients of the letter. The missing link could be the context of heavenly ascent, acceptable in contemporary Judaism, but suspect to a later generation of both Christians and Jews, who had seen this practice misused in Gnosticism. Against such a background we first looked for evidence of the χειρόγραφον being used in the sense of a note of passage, enabling the mystic to pass unhindered through the various stages of his ascent to the angel presence of God. But such evidence has not been forthcoming in contemporary literature. It is, however, present in later literature, when the ideas and terminology of Colossians had been taken up and used in a gnostic way⁽³⁷⁾.

II. A Proposed Solution

Where does this leave our survey of χειρόγραφον in Col 2,14? It would seem that we are left with the idea of a bond or certificate of indebtedness to which all men have subscribed because of their sin. There is no need to suggest that the Jews are indebted through obligation to the Torah, and Gentiles through the natural law, since the metaphor merely makes the point of the cancellation of debt. The link with the theology of mystical ascent is provided by the accompanying datival phrase τοῖς δόγμασιν. This is not a reference to the Mosaic law, so we cannot take it as depending on

(37) YATES, "Colossians and Gnosis", 59-62.

an implied γεγραμμένον, meaning 'consisting of decrees' ⁽³⁸⁾. Nor is it an instrumental dative, 'by means of the decrees', referring to what the Greek Fathers called 'the evangelical dispensation'. N. Turner ⁽³⁹⁾ makes the suggestion that this could be an adjectival dative; an example of a noun with a dative attribute, giving the sense 'subscription to the ordinances'. But which ordinances are meant? The cognate verb δογματίζομαι in Col 2,20 would seem to suggest that they are the ascetic rules, dietary regulations, and ordinances concerned with times, seasons and religious festivals referred to in Col 2,16-23. Such δόγματα are part of the required preparation for those who would experience the visionary ascent of the mystic to witness the worship of the angels in heaven. Some of the prescriptions of the law of Moses may be included in these regulations, but not the Torah as such.

By his death on the cross Christ has dispensed with the need for subscription to such ordinances, since access to God and the fulness of Christian experience is now available to all Christians through him. The author is making the point to those who would claim that the height of religious experience is to be sought in release from the flesh and ascent to the angel presence of God, that the only putting off of the flesh now required is the putting off of Christ's body in death. All believers are associated with that death in their baptism when they are incorporated into the body of Christ. Hence the great metaphor of forgiveness, the cancellation of debt represented by the cheirograph, dominates the verse.

Most people in Asia Minor at that time would be acquainted with certificates of indebtedness. They provided a useful basis for the metaphor of obligation and forgiveness, applied first to human sin, and then to the voluntary and unnecessary obligation taken on in pursuit of visionary ascent. In the cheirograph the author of Colossians has found a new way of expressing the significance of Christ's death. The sense of obligation and indebtedness associated with such bonds of debt is filled with new meaning when used as a metaphor of forgiveness, and becomes a powerful means of describing the effects of the cross.

In the light of this background for the cheirograph, the remaining grammatical and exegetical difficulties of the verse more easily fall into place. The subject of the sentence is properly God, but the close identity of the activity of Christ and God, which is evident throughout the New Testament, makes it possible for the reader to predicate the activity of removing the cheirograph (and indeed the whole of the action of 2,15) to Christ ⁽⁴⁰⁾. The description of what was accomplished on the cross more naturally implies that Christ is the subject.

⁽³⁸⁾ Apart from Eph 2,15, where it is governed by the preposition ἐν, δόγμα is never used in the New Testament of the law of Moses. Luke uses it of the decrees of the emperor (Luke 2,1; Acts 17,7), and of the decisions of the Apostles (Acts 16,4).

⁽³⁹⁾ J. H. MOULTON, *A Grammar of New Testament Greek*, Vol. III: *Syntax* (ed. N. TURNER) (Edinburgh 1963) 219. In this he confirms the suggestion of ROBINSON, *The Body*, 43, n. 1 that we should treat the dative as implied in the action of the verb 'to subscribe to' (behind χειρόγραφον) 'our subscription to the ordinances'.

⁽⁴⁰⁾ LIGHTFOOT, *Colossians and Philemon*, 189, suggests that the appropriate point of transition from God to Christ takes place at the clause καὶ αὐτὸ ἤρκεν

The twice-repeated stress on the hostile character of the certificate of indebtedness overloads the sentence. The phrase ὃ ἦν ὑπεναντίον ἡμῖν is appended to the expression 'the cheirograph which stood against us' as an interpretive clarification, leading G. Schille⁽⁴¹⁾ to suggest that it could well be a later gloss. But the words 'which was against us' need not be regarded as an awkward addition to an already existing text. The earlier phrase τὸ καθ' ἡμῶν emphasises the validity of the bond with reference to us, while ὃ ἦν ὑπεναντίον ἡμῖν stresses the active hostility of the cheirograph produced by this fact.

There is a three-fold emphasis on the cancellation or removal of the cheirograph. God has not only forgiven all our sins, he also utterly removed the signed acknowledgment of our indebtedness. The removal of the debt is signified by ἤρκεν ἐκ τοῦ μέσου (αἵρω being the main verb of the verse), and further elaborated by the figure of 'blotting out' and 'nailing to the cross'. The perfect tense signifies the permanence of the removal⁽⁴²⁾. ἐξαλείφω is used by classical writers of the blotting out of writing, especially of items in an account⁽⁴³⁾. God has wiped clean the slate and given a fresh start.

The abrogation of the cheirograph is made even more emphatic by the image of nailing to a cross. The aorist expresses the historical fact. It has been suggested that records of debt were crossed out with a letter X⁽⁴⁴⁾, but the usefulness of this analogy is questionable since in the Greek there is no play on the word 'cross'. There is also insufficient evidence for the alleged custom of piercing a cancelled bond with a nail⁽⁴⁵⁾. The reference to the title⁽⁴⁶⁾ set up over a criminal's head to indicate his crime is of limited significance. In fact only one thing was nailed to the cross, namely the body of Jesus⁽⁴⁷⁾. The figure is suggested simply by the crucifixion. It is a vivid way of saying that because Christ was nailed to the cross, our debt has been completely forgiven, rent with Christ's body, and destroyed in his death. We have a glimpse in this metaphor of how Christ's death was effective for us. When the author thinks of sin as a debt that has to be cancelled, he

because a) the participles are replaced with a finite verb, b) the aorists are replaced by a perfect, c) the substitution of ἦρεν for ἤρκεν in some MSS betrays a consciousness on the part of the scribes of the dislocation produced by the new tense.

⁽⁴¹⁾ G. SCHILLE, *Frühchristliche Hymnen* (Berlin 1965) 35.

⁽⁴²⁾ C. ROWLAND, *The Influence of the First Chapter of the Book of Ezekiel on Jewish and Early Christian Literature* (Unpublished Cambridge Ph. D. Thesis 1974) 270 suggests that ἐκ τοῦ μέσου means 'removed as a mediator'.

⁽⁴³⁾ See the literature cited by LIGHTFOOT, *Colossians and Philemon*, 186-187. Cf. Acts 3,19; Rev 3,5. There are also links with the biblical image of the cancellation of sins: Ps 51,3.11; 109,4; Isa 43,25; Jer 18,23.

⁽⁴⁴⁾ A. DEISSMANN, *Light From the Ancient East* (London rev. edn 1927) 332-333.

⁽⁴⁵⁾ F. FIELD, *Notes on the Translation of the New Testament* (Cambridge 1899) 195-196, finds no real authority for it.

⁽⁴⁶⁾ M. DIBELIUS, *An die Kolosser, Epheser, an Philemon* (Tübingen 3 1953) 31.

⁽⁴⁷⁾ Col 2,14 and John 20,25 are the only New Testament references to nails at the crucifixion, although there are many in the Fathers. See LAMPE, *Patristic Greek Lexicon*, 1171.

immediately adds that the bond is removed not by any ordinary erasure, but by being nailed to the cross as Jesus was. The metaphor is so violent as to practically rupture itself because it cannot contain the new wine of the gospel.

The kind of spirituality practised by certain of the Colossians could so easily lead to a neglect of the reality of forgiveness and of the atoning significance of Christ's death. Those who did not embrace such a spirituality would be made to feel guilty, and to think that their faith was not complete. This is why the author is so keen to lay emphasis on Christ's death as a 'putting off of the body of flesh' ⁽⁴⁸⁾, and also on a cancellation of the bond of indebtedness nailed to the cross, as was the body of Jesus. All Christians share in this. There is so much that the author wants to say in his attempt to do justice to all aspects of this metaphor of atonement that he is in danger of overloading the sentence. But he does attempt to show how Christ's death brings life to us.

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⁽⁴⁸⁾ Col 2,11.

RECENSIONES

Vetus Testamentum

Lloyd M. BARRÉ, *The Rhetoric of Political Persuasion. The Narrative Artistry and Political Intentions of 2 Kings 9–11* (The Catholic Biblical Quarterly Monograph Series 20). Washington DC, Catholic Biblical Association of America, 1988. ix-161 p. 15,3 × 22,3. \$5.00.

“The original narrative underlying 2 Kings 9–11 was, in all likelihood, an autonomous literary entity” (29); “...its composition was motivated by political and apologetic concerns” (52). “Apparently disturbed by damaging comparisons to Jehu’s coup, our author sought to exonerate Jehoiada by emphasizing the differences that distinguish his revolt from Jehu’s. In order to communicate this essential feature of the story it seems appropriate to designate it more specifically as a ‘political novella’” (ibid.). These short quotes from Barré’s study, which is a “modest revision” of his Vanderbilt University Ph. D. dissertation, summarize the main points made in his work.

Chapter I, “The Composition of 2 Kings 9–11” (4-35), establishes the unity of the original story comprising both Jehu’s and Jehoiada’s coups. A detailed analysis (8-29) argues that most of the prophetic material in 2 Kgs 9–10 must be deleted together with the Dtr and post-Dtr additions of 2 Kgs 9–11. In this way the substratum of these three chapters is proved to be one piece. This conclusion is confirmed by chapter II, “Form-Critical Considerations of the Original Narrative” (36-55). Here Barré describes the structure, intention, genre and setting of this narrative. His examination of the structure points out that both accounts are composed of the same elements, arranged in identical order: (1) the setting, (2) the instigation of the coup, (3) the extermination of the old regime, (4) the consolidation of the new regime. Barré’s definitions of genre and intention have already been mentioned above. As for the setting, it is located in the Judean court, in “the early years of Joash’s reign” (52) when royal scribes tried to “convince the elders of Judah that Jehoiada’s coup, unlike Jehu’s, was a heroic and righteous act of loyalty to the Davidic dynasty” (54).

Next comes chapter III, “An Expository Reading of the Original Narrative” (56-98) and chapter IV, “The Redaction of 2 Kgs 9–11” (99-138); the latter explains in detail the contribution of the redactors. Chapter V, “Conclusions” (139-143) is followed by an adequate and

up-to-date bibliography (145-153). An index to scriptures is provided (155-161).

Barré's study has more than a few merits: it is lucid, well argued and not cumbersome. He avails himself of the various directions of biblical study: historico-literary criticism, form-criticism and close reading. He is versed in the history of research and most of his single comments to the text make good sense; an exception is, in my opinion, his acceptance of Parker's explanation (in *Maarav* 1 [1978] 67-78) of Jezebel's conduct (2 Kgs 9,30-31) as meant to seduce Jehu (61, 73, 76-79). If his theses fail to convince me, this is not due to lack of ability on his side, but to a lack of plausibility in the theses *per se*. The following remarks are meant to substantiate this statement.

The unity of 2 Kgs 9-11 depends very much on the detraction of the prophetic elements from the account of Jehu's coup (2 Kgs 9-10), because 2 Kgs 11 is a 'non-prophetic' account. Barré is justified, indeed, in considering some of these passages (such as 2 Kgs 9,7-10a) as secondary. I wonder, however, whether this applies to all prophetic quotations. A case in point is 2 Kgs 9,25b-26. It contains a singular prophecy, unparalleled by anything told elsewhere in the Omrides' stories. In my view, in this case as elsewhere in the Bible, peculiarity of style and contents is the hallmark of originality. As a second argument Barré adduces: "it is unlikely that the original narrative, concerned as it is with picturing the rapid pace and secrecy of Jehu's assault... would have included the scene described in vv. 25-26" (14). However, the picture of Jehu's usurpation implies also his supreme interest to legitimize his coup by relying on the Lord's word against Achab. Moreover, one should take into account that the literary stylization could have dictated at this point a shift in the order of events; cf. N. Lohfink, "Jona ging zur Stadt hinaus (Jona 4,5)", *BZ* (NF) 5 (1961) 185-203.

Thus, if the prophetic element is present in some original passages of 2 Kgs 9-10, it differentiates this story from the one about Jehoiada's revolt in 2 Kgs 11. The unity of both accounts does not stand the test. But even if Barré were right in considering this element secondary everywhere, how would he account for its being introduced in the Jehu story only? In my opinion, the two revolution-reports appear to have had different literary origins and history.

Indeed, Barré points out that both accounts have a similar fourfold structure — setting, instigation etc. Yet, one wonders: is this structure a literary pattern? is it determined by the author(s)? More plausibly, to my mind, the 'structure' was dictated by the events: every revolt must have a setting and an instigation, and if it succeeds, extermination of the old regime and consolidation of the new one follow. We are allowed to assume literary patterns, only if the pattern can be proved not to originate in reality. All the more so when we deal with events that took place in twin states in the span of seven years.

In my view, the genre-designation of 2 Kgs 9-11 as a 'political novella' glosses over the distinct character of each of the chapters. Ch. 9 is a vivid, very detailed account, animated by dialogues and, consequently,

rich in characterization. One senses here a pleasure at narrating, a novelistic flavor. By contrast, ch. 11 is a sober, terse account, reticent in characterization; its speeches convey instructions, not feelings and passions. The one exception is Athaliah — she is passionate. The genre of this account is kindred to annalistic or chronistic writing, political but not novelistic. Ch. 10, again, is something quite different: its two main episodes, vv. 1-10a and vv. 18-27, are no history at all; they are anecdotes, namely short invented stories that condense in one episode the character and feats of famous persons. Anecdotes tell things indefinite, vague and bizarre; true, they are not easily recognized, especially in the Bible. Yet, would one doubt that the stories about David's feigned madness (1 Sam 21,11-16) and Solomon's trial (1 Kgs 3,16-28) are anecdotes?

The classification of literary works according to genres is admittedly an arbitrary procedure (cf. M. Weiss, *The Bible from Within* [Jerusalem 1984] esp. 54-67). Therefore, the divergences between author and reviewer in defining the genre(s) of 2 Kgs 9-11 come as no surprise. Yet, the main point of my criticism lies not in the results of the work, but rather in its method. The type of intellectual activity (what Jolles called *Geistesbeschäftigung*) that brought into being a given literary opus should be seriously taken into account. In this way we may be able to sharpen our perceptions and distinctions about the nature of Israelite historical and fictional literature.

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K. NIELSEN, *There is Hope for a Tree*. The Tree as Metaphor in Isaiah (JSOT Supplement Series 65). Sheffield, Sheffield Academic Press, 1989. 301 p. 14 × 22. £ 20.50 / \$31.95.

El autor estudia en este libro el uso de imágenes vegetales en el complejo Is 1-39. La imagen vegetal es en sí polivalente; lo que cuenta es apurar el uso sucesivo o diacrónico de cada imagen, sea en textos diferentes (como Is 5,1-7 y 27,2-6), sea en etapas redaccionales del mismo texto (como 10,33-34). El libro se desarrolla en tres partes: una reflexión sobre el método (p. 13-68), la imagen del árbol en su contexto cultural (71-86), análisis de doce textos. Siguen la conclusión y los índices de costumbre. Voy a fijarme especialmente en el aspecto metodológico.

Es una lástima que el autor no haya comenzado directamente tratando de las imágenes, tema específico de su tesis. Antes de llegar ahí nos conduce por una serie de observaciones sabidas y admitidas. (No sé por qué les exigimos a los autores de tesis empezar cada vez por el abecé de nuestra disciplina.)

Define la imagen como fenómeno interno al lenguaje, por interacción de dos componentes: la interacción sucede en el lenguaje, el sentido apunta

a la realidad (Ricoeur distingue y defiende la explicación sintagmática frente a la paradigmática). Por la interacción se produce una iluminación mutua de ambos componentes (Bruno Snell lo había descrito en su libro *Die Entdeckung des Geistes*, p.190s, siguiendo a Hans Lipps, *Die Verbindlichkeit der Sprache*). Hay que distinguir la metáfora activa, la lexicalizada (*dead metaphor*) y la actualizada, cosa no siempre fácil en textos antiguos. No basta estudiar la metáfora como género — fábula, parábola, alegoría —, sino que hay que estudiarla como estilema o procedimiento.

Son observaciones orientadoras. Pero me parece que no distingue bastante entre metáfora, concepto específico, e imagen, concepto genérico; la mayoría de los textos estudiados no son metáforas, sino imágenes. Tampoco entiendo que la metáfora «consista en dos enunciados» (*two statements*).

Sigue una aplicación a la metáfora de las teorías de Austin y Searle sobre lenguaje performativo e ilocución. «Característica de las imágenes es la relación dialéctica entre las funciones informativa y performativa». No creo que eso pertenezca a la imagen en cuanto tal, sino más bien a la ilocución propia del contexto. El ejemplo aducido de Is 5,1-7 lo muestra a las claras: la fuerza de interpelación no procede de la imagen, sino del imperativo «fallad el pleito...», del salto de la lírica a la retórica (lo indico en *Profetas* I,134). Sigue exponiendo la distinción entre lenguaje imaginativo y literal (mejor, figurado y propio): estudiando textos antiguos no es fácil apelar al uso común. Lo que dice allí sobre la ironía (63) ya lo habían resuelto los retóricos clásicos con su distinción entre tropos y figuras, figuras de lenguaje y de pensamiento.

En los análisis individuales dedica la mayor parte del esfuerzo a explicar la génesis y proceso de los textos, resumiendo cuando hace al caso opiniones ajenas antes de proponer la propia. Este método tiene, en mi opinión, dos inconvenientes serios. Primero, restar espacio al estudio específico de las imágenes e hipotecarlo con conjeturas (mientras no tengamos más datos ciertos o muy probables, cada nuevo intento de explicación genética, en vez de hacer avanzar nuestra comprensión del texto, nos carga con una conjetura más). Segundo, no se estudia bastante la función de la imagen en el poema como macroestructura. P.e. el retoño de Is 11,1 en el sistema simbólico del mundo vegetal (vitalidad), animal (reconciliación), de elementos (aire = *ruh* y agua del mar), humano (bebé, niño pequeño, en armonía con brote y cría de animal).

No está clara la distinción entre significación y denotación o identificación. P.e. los árboles copudos y frondosos significan lo soberbio y arrogante, pueden referirse a Asiria o a Jerusalén.

Echo de menos al comienzo de cada análisis una traducción justificada del texto. Así p.e. en Is 27 no nos aclara la relación entre el sujeto masculino de *ym*, *y'sh* y el complemento femenino.

Respecto a la presentación que hace de mi pensamiento (un libro incompleto y cuatro artículos), observo lo siguiente: Habiendo publicado desde entonces bastantes comentarios breves y extensos, numerosos artículos y recientemente (1987.88.89) el *Manual de poética hebrea*, encuentro la presentación «arqueológica» y también incompleta, ya que «*Tres imágenes de*

Isaías» y «*Dos poemas a la paz*» datan de 1956 y 1959 (recogidos ahora y revisados en *Hermenéutica de la palabra II*).

El autor tiene bien presentes varios datos del contexto cultural, especialmente temas míticos, cultos de fertilidad, ideología regia. Retornan con frecuencia en la exposición y en oportunos excursos. Otra importante constante temática es la referencia política a reinos y gobernantes. El índice de citas bíblicas comprueba que el autor tiene en cuenta también el ancho contexto bíblico de la imagen del árbol.

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J. A. DEARMAN, *Property Rights in the Eighth-Century Prophets* (SBL Dissertation Series 106). Scholars Press, Atlanta, Georgia, 1988. x-171 p. 14 × 21,5.

Reproduce esta obra la tesis realizada por el autor en 1981 bajo la dirección de John H. Hayes. El tema, como deja claro el título, se centra en «las referencias a los derechos de propiedad en la literatura profética del siglo VIII, en un intento de entender estas referencias dentro del contexto socio-económico de la historia de Israel» (1).

El autor estructura su estudio en una introducción y seis capítulos. La Introducción comienza aclarando brevemente los términos de propiedad y de derecho; luego ofrece una panorámica de la investigación sobre la crítica social en los profetas, desde finales del siglo pasado hasta la fecha de la tesis, con especial atención a las opiniones de Walter, Alt, Koch y Loretz (dado el enfoque de su trabajo, pienso que debía haber concedido más atención a la teoría de M. Fendler).

El capítulo primero (17-61) estudia los profetas del siglo VIII (Amós, Oseas, Isaías, Miqueas), intentando responder a dos preguntas básicas: ¿cuáles son los derechos fundamentales o primarios a los que se hace referencia cuando se habla de propiedad? ¿Quiénes son responsables, si los hay, de su violación o administración? Para ello analiza Am 2,6b-8; 3,9-10; 5,11a; 7,1b; Os 4,2a; 5,10a; 7,1d; 12,8-9a; Is 3,12.13-15b; 5,8; 10,1-2; Mi 2,1-2; 2,8b.9; 6,16a. El estudio de estos textos lleva a Dearman a tres conclusiones preliminares: 1) la responsabilidad del conflicto no recae sólo en la codicia de los ricos, sino también en las autoridades, a las que, por otra parte, no se acusa de prácticas cananeas; 2) existe estrecha relación entre la administración de la justicia y la opresión económica; 3) los textos reflejan un conflicto entre los derechos de los ciudadanos y los de los administradores y oficiales del estado. El capítulo termina ofreciendo dos esquemas, uno sobre los textos del Pentateuco y los derechos de propiedad, y otro sobre las autoridades nacionales y la perversión de la justicia.

El capítulo segundo se centra en la tenencia de la tierra y los derechos de propiedad (62-77), encontrando en el Antiguo Testamento dos puntos de

vista: uno conservador, probablemente agrario, que intenta mantener la propiedad inmueble como herencia de familia, y otro de orientación más comercial, que acepta que el derecho de posesión incluye el de alienación o venta. Tras analizar los datos de otras culturas (Mari, Nuzi, Ugarit y Grecia), revisa los textos proféticos sobre el tema con las siguientes conclusiones: 1) la apropiación de campos se debe principalmente a las deudas de los campesinos; 2) la crítica profética presupone una ley consuetudinaria orientada a proteger la propiedad ancestral, pero no puede ser usada como evidencia de unas leyes que exijan no alienar los campos; 3) esta ley tiene una base yahvista; 4) no es exclusiva de Israel, sino que tiene paralelos en otras culturas; 5) nada demuestra que se produjese un cambio desde un *Bodenrecht* israelita a otro de orientación cananea.

El tercer capítulo se centra en los derechos de propiedad y el sistema administrativo/judicial (78-107). Es un estudio de la responsabilidad de dicho sistema en la pérdida de los derechos de propiedad denunciada por los profetas. Analiza, además de los textos proféticos, Ex 18,13-27; Nm 11,16-17.24-30; Dt 1,9-18; 16,18-20; 17,8-13; 2 Cr 19,4-11, especialmente relacionados con el tema de la organización de la justicia. Dentro del contexto de la obra, una de las principales conclusiones es que nada sugiere que «los administradores, la estructura o la política (de este sistema) sea específicamente «cananeo» y, por tanto, una perversión de la costumbre israelita» (103). El capítulo termina con un excursus sobre la carta de Yabneh Yam y un esquema sobre el sistema administrativo/judicial, enumerando los títulos de los oficiales y las obligaciones que les competen.

El capítulo cuarto, centrado en «Los oficiales reales y los derechos de propiedad» (108-131), discute con más detalle los privilegios del rey y de sus oficiales, centrándose en las concesiones de tierras y en los ingresos por impuestos. Tras analizar la tradición contenida en 1 Sam 8,11-17 (que considera una parodia exagerada, pero interesante para captar ciertos aspectos históricos, basados en la política de Salomón más bien que en las prácticas de la monarquía cananea) completa el panorama con los ostraca de Samaría y las jarras judías con el sello *lmlk* y con un excursus sobre las prácticas administrativas neosirias.

El capítulo quinto trata el «Trasfondo del conflicto sobre la propiedad» (132-147), distinguiendo factores internos y externos y analizando la situación en Israel y Judá. Lo que me ha resultado de mayor interés es que como modelo alternativo a los usados a veces de «capitalismo temprano» o «capitalismo de rentas» adopta el de «sistema de redistribución», basado en McClellan.

El capítulo final se limita a dos páginas de conclusiones (148-149), de las que entresaco las que considero más importantes: 1) No hay motivos para aceptar que los profetas se oponían a prácticas cananeas o a oficiales cananeizantes. 2) No hay motivos para aceptar que Israel o Judá tuviesen una economía capitalista, en el sentido moderno de la palabra. 3) La idea de que el *Bodenrecht* israelita prohibía vender la propiedad ancestral es equivocada. 4) Las denuncias proféticas presuponen que el sistema administrativo y judicial es el principal responsable; tales oficiales ocupan también en ocasiones un puesto militar.

El estudio termina con una bibliografía (151-171) que, por imperativos de la serie, sólo abarca hasta el año 1981, fecha en que se presentó el trabajo.

La lectura de la obra me ha producido una agradable impresión, a lo que contribuye en no poco la claridad del desarrollo y los resúmenes que ofrece Dearman al final de cada capítulo. Además de la claridad, es digno de todo elogio el análisis objetivo, falto de prejuicios, de los textos, y el no eludir ninguno de los problemas importantes. Si bien el capítulo primero me ha parecido que trata demasiado aprisa textos tremendamente complejos (para ello me remito a mi libro «*Con los pobres de la tierra*». *La justicia social en los profetas de Israel* [Madrid 1985]), los siguientes estudian los temas de forma bastante completa e interesante. Estoy básicamente de acuerdo con todas sus conclusiones. En todo caso, diría que Dearman carga excesivamente la responsabilidad de las injusticias en las autoridades, salvando demasiado a los ricos. Un análisis de todos los textos proféticos sobre el tema de la injusticia (no sólo de los relacionados con el derecho de propiedad), y el sentido común más elemental, impulsan a pensar que los ricos propietarios del siglo VIII, fuesen muchos o pocos, tendrían también parte de culpa en el problema. Dearman no lo niega, incluso lo sugiere a veces, pero quizá exagere en su enfrentamiento con esa mentalidad superficial que sólo ve en la denuncia profética una lucha de ricos y pobres. Es una pena que este trabajo haya tardado siete años en publicarse; a pesar del tiempo pasado, lo considero de gran interés, y deberán consultarlo todos los interesados en el tema.

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Novum Testamentum

Joachim KÜGLER, *Der Jünger, den Jesus liebte* (Stuttgarter Biblische Beiträge 16). Stuttgart, Katholisches Bibelwerk, 1988. 518 p. 15,5 × 23,3.

J. Kügler's dissertation, directed by P. Hoffmann and accepted in 1987 by the Catholic Theological Faculty at the University of Bamberg, is here presented in slightly revised form. It addresses itself to several issues, some old and others new, which in recent years have emerged as important components in an ongoing, remarkably vigorous discussion of the Fourth Gospel. What is old is the perplexing question of the identity of the "Beloved Disciple" (hereafter BD), while what is new revolves largely around the newer literary methods of interpretation, specifically as they are applied to the BD and his potentially paradigmatic function in the Fourth Gospel.

Kügler offers a systematic analysis of BD passages found in chaps. 13, 19, 20 and 21. (Other passages are treated, such as 1,35-40 and 18,15-16,

including a lengthy excursus on 6,26-71.) His findings in the BD passages may be briefly summarized as follows: the BD has authority in identifying unbelievers and false disciples (chap. 13). In this chapter, Judas functions as the representative of all unbelievers. (Kügler uses this interpretation as a key in understanding the significance of the defectors in 6,66 and the discussion with the disciples that follows in 6,67-71.) Moreover, the BD is Jesus' representative on earth, and is an eyewitness of his death (chap. 19). The BD is also an eyewitness of the empty tomb (chap. 20), thus making him the "reliable [*zuverlässige*] authority for correct interpretation of the facts of salvation" (432). According to chap. 21, whereas Peter is the shepherd of the flock, the BD is the "witness" (*Zeuge*) upon whom Peter has to rely (433). All of this makes the BD a chief witness, ranking him first in "faith and witness" (475).

Of related interest is Kügler's interpretation of the Paraclete passages. He argues that there are deliberate and meaningful parallels between the BD and the Paraclete: Both represent Jesus after his departure; both bring to remembrance what Jesus had said; both speak the truth; and both want to lead the disciples into all truth (436). Although not identifying the BD with the Paraclete, Kügler believes that the Fourth Evangelist has presented the BD as the one who fulfills the Paraclete's work and ministry. Such parallels would only enhance the status and authority of the BD.

But was this BD a historical person? And, if so, who was he? Agreeing with P. Parker's influential study (*JBL* 81 [1962] 35-43), Kügler rejects the traditional identification of the BD with John the son of Zebedee. He also rejects the various identifications with John "the Elder", John Mark, Lazarus, and others. Such attempts at identification are in vain, for Kügler believes that the anonymity of the BD is deliberate — not to hide the identity of a historical person, but to present the BD as the ideal disciple. The BD, Kügler avers, "was not a self-portrait of an apostolic author", but a fictional "guarantor of old theological tradition", a "personification of the apostolic origin of Johannine Christianity" (486). The BD is an authoritative witness to major "events of salvation" (*Heilstaten*). Consequently, the BD passages are oriented to the "extratextual world" (*außertextliche Welt*), that is, oriented to the reader. These passages draw the readers into the story, not to inform them of what happened or who the BD was in history past, but to tell them what it means to be a true disciple of the risen Lord.

As to the *Sitz im Leben* of the Johannine community, Kügler detects the presence of a schismatic group that has rejected the necessity of the Lord's Supper. This they have done because by believing in Jesus salvation has been accomplished. There is therefore no need to consume the bread of the eucharist. This is the major issue, Kügler believes, and to this issue chap. 6, the *Brotrede*, speaks. It is the issue around which the Fourth Gospel revolves.

There are features of Kügler's exegesis that strike me as compelling. His description of BD as the ideal disciple appears to be essentially sound. His scrutiny and interpretation of the various relevant details usually appear careful and sound. A recent study by W.S. Kurz (*BTB* 19 [1989]

100-107), who of course had not yet seen Kügler's study, reaches a conclusion similar to Kügler's: the BD functions as a paradigm of the ideal disciple, a disciple that the Fourth Gospel's implied readers are to emulate.

Two of Kügler's principal conclusions, however, are problematic. First, I have difficulty with his literary analysis, in which he concludes that the BD is a symbol and nothing more. Admittedly the identification of the BD with John the son of Zebedee, or with any other specific candidate, is problematic. I must nevertheless agree with R. E. Brown, who has said: "The thesis that he [the BD] is purely fictional or only an ideal figure is quite implausible. It would mean that the author of John 21,20-23 was deceived or deceptive, for he reports distress in the community over the Beloved Disciple's death. The Disciple was idealized, of course; but in my judgment... he was a historical person and a companion of Jesus" (*Community of the Beloved Disciple* [1979] 31). The report of the death of the BD simply makes no sense in Kügler's interpretation. Granted that the BD is presented as the ideal disciple, why must that fact militate against the identification of this disciple with John or with some other historical disciple? Could not a historical personage be presented as an ideal figure, as in fact Origen had thought? (See Brown, *Gospel according to John XIII-XXI* [1970] 924.) Is this not what, in a certain sense, the author of Hebrews has done? Jesus is presented as the ideal Priest and Sacrifice, yet his life and death on the cross are quite historical. (For other recent studies that grapple with the identity of the BD, "John the Elder", and the authorship of the Fourth Gospel see P. Grelot, *RB* 94 [1987] 519-573; D. Muñoz León, *EstBib* 45 [1987] 403-492; J. K. Thornecroft, *ExpTim* 98 [1987] 135-139; B. Bonsack, *ZNW* 79 [1988] 45-62.)

Secondly, Kügler's interpretation of chap. 6, along with his proposed *Sitz* based on this interpretation, is also problematic. The "Bread of Life" discourse, admittedly central (see Grassi, *CBQ* 48 [1986] 67-80), is but one of several other "I am" discourses. In what ways do these discourses contribute to our understanding of the Johannine *Sitz*? If they do not, then how does Kügler know that chap. 6 points to the real problem? There are simply too many facets to Johannine theology and polemic to reduce to this theme. If the eucharist lies at the heart of the matter, then why is so little made of it in chap. 13? How does the material concerned with being expelled from the synagogue fit into this proposed *Sitz*? Does the division between believers and unbelievers really reduce to those who eat the bread (i.e., the BD and all other true believers) as opposed to those who do not (i.e., Judas Iscariot and the unbelievers)? I cannot help but wonder if later, more developed eucharistic theology is being read into the Fourth Gospel. Other troublesome questions could be asked.

Although there is much in Kügler's book with which I disagree, it is indeed a stimulating study, one that has raised some important questions, and one that will no doubt lead to further discussion and progress.

The book contains no Scripture index, which is an inconvenience. I often found myself flipping through the pages trying to find what was said

about such-and-such a passage. The bibliography is excellent; Kügler has cited and made good use of the major works found in English and French, as well as in German.

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Stephen WESTERHOLM, *Israel's Law and the Church's Faith. Paul and his Recent Interpreters*. Grand Rapids, Eerdmans Publishing Company, 1988. 238 p. 13,5 × 21,2. \$14.95.

Confronted with the vast body of scholarly literature on Paul, the interested lay person and the student scarcely know where to begin. Dr Westerholm sets out to aid them by offering an overview of the debate on a crucial area of Pauline thought, hoping that scholars may profit from the enterprise as well. How many lay persons are prepared to follow Westerholm all the way through the labyrinth I do not know, but at least for the student and for his own fellow scholars he is an excellent guide.

The book falls in two parts. The first presents a dozen scholars "whose works mark important stages leading up to and shaping the current debate"; the second deals systematically with Paul's view of the law.

It was a good idea to start with Luther, whose interpretation of justification continues to exert an immense influence on Pauline study. In view of another popular current, Calvin's position (appealed to, e.g., by Cranfield) might also have been mentioned. Westerholm then presents various challenges to the "Lutheran" view: the relegation of justification by faith to the periphery (Wrede, Schweitzer); the recognition that Judaism has been misrepresented (Montefiore, Schoeps, Sanders); the changed view of Paul's conversion (Kümmel, Stendahl); Bultmann's Lutheran notion of "works" and its refutation by Wilckens and Sanders; the questioning of Paul's consistency (Drane, Hübner, Räisänen).

The views of all these scholars are carefully described here. Of course, any selection raises questions about who is included and who is not. As a precursor of the challenge to Luther, F. Chr. Baur, who stressed universalism rather than justification by faith, might have been worth mentioning. On the other hand, has Drane's work shaped the current debate more than that, say, of W. D. Davies? Part Two starts with a debate with those who deny that Paul abandoned the law. Their position is convincingly refuted, but no hint was given in Part One that such a view is still alive and well.

Since Part One consists of descriptive reports, most of the authors presented there never get a critical evaluation. It might have helped the beginner's orientation to learn that Schweitzer suggested several explana-

tions for Paul's view of the law which may not be compatible with each other, or that Drane's reconstruction hinges on his particular dating of Galatians. Of the scholars who appear in Part One, only Sanders and myself (and, to some degree, Bultmann) are treated as partners still engaged in discussion in the second part. As the book focuses on the present challenge to the "Lutheran" view, this may be all right; yet the portraits in Part One could have been related more effectively to each other. For instance, long before present-day questioners of Paul's consistency, Wrede pointed out contradictions and artificial features in his letters (cf. 21f).

Westerholm's exegesis in Part Two is characterized by clarity, common sense and wit. He is exceptionally fair in his treatment of scholars from whom he differs, including myself. The book is a model of responsible communication.

Westerholm's analysis results in an interesting combination of the "Lutheran" picture with the "new perspective on Paul" (Dunn's term for the shift inaugurated by Sanders). Paul does stress divine grace as opposed to human effort, and this is where he sees the difference of Christianity from Judaism (the Lutheran view). But in doing this, Paul does not take Jewish self-understanding as it was in his time as his starting point, but his faith in a crucified Messiah. Westerholm agrees with Sanders (against the Lutheran view) that Paul's reasoning runs backwards, "from the solution to the plight". For Paul, the offer of salvation made in the gospel competed with the promises of the Torah. He was therefore forced to account in retrospect for the Torah's failure in one way or other.

One of the consequences of this is Paul's more pessimistic view of man. While Westerholm agrees with Sanders that the idea of universal transgression was not Paul's starting-point, he is not content to regard that idea merely as an argument to prove a point, but wants to take it seriously (in the Lutheran vein) as part of Paul's developed view. There is some force in this contention.

In determining Paul's relation to average Judaism on "works", Westerholm comes, despite initial criticisms, pretty close to my position. Paul does contrast grace and works in a way that suggests that Judaism worked with the same distinction but argued for salvation by works, and thus he "distorts Judaism" (against Sanders and Dunn who think that Paul does not differ from normal Judaism in his assessment of works). On the other hand, Paul's judgment implies no caricature of Judaism either (150). "Observance of the law may be regarded as Israel's path to life" and the law thus had a "soteriological function" in Judaism. "If Paul is wrong in considering the law a path to salvation, it is an error he shares with Leviticus, Deuteronomy, and Ezekiel" (147). But here the key question is, what does "path" really mean? Is it identical with a "means" of salvation? Does the law belong to the "indicative" or to the "imperative", or are such distinctions simply academic? In any case, Westerholm's careful and nuanced analysis confirms that it is Paul who drives a wedge between grace and works which belonged together in "normal" Judaism. "Paul must not be allowed to be our main witness for Judaism", but neither must Judaism "determine the limits within which Paul is to be interpreted" (150).

Westerholm likewise concedes to me (and Sanders) that Paul's statements on the law are free neither from inconsistencies nor from problematic assertions, although he restricts their number and suggests that attention to them prevents me from seeing the grandeur of Paul's theological contribution. And he may be right. It is a matter of taste and perspective.

Westerholm admits that the problems include Paul's question-begging conviction that faith in Christ implies inadequacy of the law, which in turn forces him to seek explanations for the place of the law in God's design. Westerholm repeatedly defends Paul by pointing out that when Paul presents problematic explanations he is doing nothing novel. Problems there may be, but Paul cannot be blamed for them. His exclusively christocentric starting-point for instance was given in the pre-Pauline creed that "Christ died for our sins".

The problem of theodicy implicit in Paul's argument is not peculiar to him either; "we do him an injustice if we regard his synthesis as less adequate than that of others who do not even face the issue". I accept this criticism of my work. In fact, I had already proceeded in the same direction, noting that in so far as Paul seems more inconsistent than other early Christian writers, it is mainly because he devotes more attention to the problem. "Hard pressed between the claims of sacred tradition and the vision triggered by new experiences... he tried to do justice to both. But the situation that forced him to try that was not created by him" (H. Räisänen, *Paul and the Law* [Tübingen 1987] xxvi).

To recognize that Paul is not responsible for creating a problem does not remove it, however. Westerholm defends Paul by referring to such cases as the Fall or the death of Christ for sin conceived as part of God's eternal plan. "Most readers of Genesis 2-3 would believe that God gives there a command (2,17) — all the while knowing that it will be broken and having in mind the remedy to follow" (135). Yes — and this is one of the reasons why a modern reader cannot find the account of the fall convincing either, if conceived in (salvation-) historical terms as an account of a real change in the human plight. One person's problems cannot be solved by referring to others involved in analogous difficulties. But surely many Pauline dilemmas should be taken less as personal idiosyncrasies than as pointers to general theological problems which arise when sacred tradition is reinterpreted in the light of new experiences (and which, I am afraid, defy solution on an "objectified" level).

Westerholm repeatedly appeals to Old Testament precedents in order to let Paul's devices appear in a less problematic light. I am not overly impressed by these analogues. Westerholm finds Paul's account of the Torah's failure "anticipated in the prophetic charge that Israel, by breaking its covenantal obligations, had rendered the Sinaitic covenant obsolete" (221). Unlike most of his conclusions, this one is not supported by adequate analysis; this would even require a discussion of the dating of the covenant idea (which may be later than the prophets). It is very one-sided to infer from Exod 20,5, Lev 26 and Deut 28 that "the negative side of the law's sanctions has become its sole practical effect" (185). The negativity of Exod 20,5 is by far outweighed by the promise in 20,6; and the deuteronomic theology of

history in Lev 26 and Deut 28 amounts to a great paraenesis in which the blessing promised in the law remains a real possibility. I fail to see that Israel's transgression is regarded as the "effect" of the Torah in the Old Testament. The reference to Jer 31 (163f) is not very instructive, for according to it nothing is expected to happen to God's law when God changes the hearts of men.

If something of importance is built on these and other OT passages, then they, too, would have to be analyzed, not just mentioned as proof-texts. It is not enough to pick up one line of interpretation either (Westerholm 163, n. 56 appeals to von Rad and his followers); rather, the pros and cons would have to be set out with the same rigour with which rival interpretations of Pauline texts are discussed in the book.

Westerholm defends Paul's argument in Rom 1-3 by referring to the prophets (158). If Paul's denunciations are faulted as too sweeping, what are we to say of passages like Jer 6,28; 8,6; 9,2-6? I think we are to say just the same. R. P. Carroll comments on the "domain assumptions" of the prophets, of which a classic example "must be Jeremiah's belief that everybody in Jerusalem and Judah was completely corrupt, wicked and incapable of becoming otherwise". This was a prejudice, "resistant to contrary evidence", for "Jeremiah was often saved from near disasters by the intervention of such 'corrupt' people" (R. P. Carroll, *When Prophecy Failed* [London 1979] 12). This time, too, problems are not removed by an appeal to the appearance of similar problems elsewhere.

As for the origins of Paul's view of the law, Westerholm agrees with me and others in assuming that Paul persecuted Hellenistic Jewish Christians because of their laxity toward the ritual demands of the Torah. However, he criticizes my position in that he finds it "hard to believe that the practice of Hellenistic Jewish Christians could have dictated Paul's conduct without — from the very beginning — stirring his thought" (217).

Once more, the agreement between Westerholm and myself would have proved even greater than it now seems, had the second edition of *Paul and the Law* been available to him (see there pp. XVIII-XIX). I have in another connection developed the view that, contrary to my older hypothesis, the "Hellenists" already had a theological *raison d'être* for abandoning the "ritual" law: a "spiritualized" view of the Torah which Paul adopted (H. Räisänen, *The Torah and Christ* [Helsinki 1986] 246-306; reprinted in *Jesus, Paul and Torah: Collected Essays* [Sheffield 1990]). Thus Paul's thought on the law was "stirred" from the very beginning, although it was only later that he developed his peculiar negative criticisms. A somewhat similar picture is now presented by Jürgen Becker in his pioneering reconstruction of Antiochian theology (J. Becker, *Paulus* [Tübingen 1989] 107-119).

Westerholm has written an outstanding book which serves to clarify the scholarly discussion of a central subject. It should be read and pondered by all who are interested in the study of Paul.

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Duane Frederick WATSON, *Invention, Arrangement, and Style: Rhetorical Criticism of Jude and 2 Peter* (SBL Dissertation Series 104). Atlanta, Scholars Press, 1988. xii-244p. 14 × 21,3. \$20.95.

This book is a Duke University Ph.D. dissertation defended in 1986 under the direction of Franklin W. Young; the author also studied Graeco-Roman rhetoric with the renowned classicist George A. Kennedy at the University of North Carolina at Chapel Hill. The author is a United Methodist minister who has recently become an assistant professor of religion at Malone College in Canton, Ohio, USA.

The book begins with a statement of the history and rationale for rhetorical criticism of biblical texts; like other rhetorical critics, Watson accepts James Muilenberg's critique of form criticism (*JBL* 88 [1969] 1-18). Like Betz, Kennedy, Jewett, and others including the present reviewer, Watson interprets NT texts using a theory of rhetoric drawn from Graeco-Roman rhetorical traditions. Watson undertakes "to provide a complete rhetorical analysis of Jude and 2 Peter according to the Greco-Roman system of rhetoric" (7), and to examine the thorny problems of the literary unity of 2 Peter and the literary relationship between Jude and 2 Peter. The author posits that his rhetorical analysis of these two documents can contribute to the solution of the literary problems of Jude and 2 Peter.

In his definition of rhetorical criticism, Watson utilizes Kennedy's five-step process for performing rhetorical criticism, as stated in *New Testament Interpretation through Rhetorical Criticism* (Chapel Hill-London 1984). This method includes the determination of the rhetorical unit, the analysis of the rhetorical situation, determination of the species (*genus* of rhetoric), the question, and the *stasis* of the question, and the analysis of invention (the choice and use of topics), arrangement and style. These analytical steps Watson carries out on 2 Peter and Jude with admirable thoroughness and skill. Notable is the fact that Watson's treatment of the two letters in question includes analyses of the smaller tropes and figures of rhetoric (which were generally thought of in antiquity as part of style), as well as analyses of topics and arrangement. Watson analyzes these three aspects of literary criticism, whereas some other recent rhetorical analyses of NT letters focus only on invention and arrangement, or on arrangement alone.

Dr. Watson's analyses of 2 Peter and Jude do not treat rhetoric alone. Rather, the rhetorical analysis provides the framework in which other relevant questions are asked and answered. Particularly welcome to this reviewer are the parts of the analyses of both letters in which rhetorical criticism and more traditional historical criticism go hand-in-hand, especially in the *exordia* of both letters which deal with eschatology or NT textual problems. One need not doubt the technical facility with which Watson deals with more traditional exegetical matters. The footnotes give good coverage of the secondary literature on these problems, such as textual variants in the Septuagint and form-critical matters, as he deals (for example) with the rhetorical value of quotations from the Old Testament.

The fact that this dissertation tries to do so many different things with two different NT letters means that the book is very technical in nature. The author does not step out of his scholarly *persona* to suggest the meaning of the NT texts in question for contemporary Christian spirituality or life. Some readers interested in the methodology of rhetorical criticism might regret the fact that Dr. Watson concentrated so heavily in the direction of analyzing the rhetorical texts of 2 Peter and Jude that literary aspects of these texts tend to overshadow the reconstruction and explication of the rhetorical situations which evoked them. One might reply that it was in the nature of this dissertation to focus more on the rhetorical text than on an analysis of the rhetorical situation, however crucial that might be. Thus, it would be more accurate to point out that any successful rhetorical analysis must have a goal or aim; Watson's goal was to determine the extent to which rhetorical analyses may contribute towards answering the perennial questions of the literary dependence and integrity of Jude and 2 Peter. True to his promise to analyze "every detail" (8) of rhetoric in these two letters, Watson sometimes states the obvious in order to lay a firm foundation for his subsequent discussions of very technical matters. Given how his dissertation's investigation was structured, he could not have done otherwise.

Several points which relate to the rhetorical criticism of letters are worthy of comment here. In particular, for those readers who might be tempted to skip over Watson's many references to the multiple ancient handbooks of rhetoric, and who are not intimately familiar with the cross-currents in the history of Greek and Latin rhetoric, it might be easy to come to the false conclusion that Graeco-Roman rhetoric was a single, rather tightly unified system. In order to introduce most New Testament colleagues or others to the complexities of different systems of rhetoric, everyone who attempts to systematize rhetoric (including the present reviewer) runs the risk of making Graeco-Roman rhetoric look more unified or uniform than it really is likely to have been, in actual practice. Further, modern systematizers of rhetoric often end up orienting their system towards a major ancient synthesis of rhetoric. Richard Volkmann's *Die Rhetorik der Griechen und Römer in systematischer Uebersicht* (Leipzig ²1885) was heavily oriented towards pre-Socratic rhetoric, a perspective largely shared by Josef Martin's *Antike Rhetorik: Technik und Methode* (München 1974). In contrast, Heinrich Lausberg's *Handbuch der literarischen Rhetorik* (München ²1974), a book which has been extensively used by several NT rhetorical critics, tends strongly in the direction of Quintilian, for which a very appropriate warning was given by A.E. Douglas in *Classical Review* NS 12 (1962) 246-247. Although Watson acknowledges his dependence on Quintilian's "mature theory" in connection with his discussion of *stasis* system of rhetoric (11-12, n.90), one can see that, in general, Watson's reconstructions of Graeco-Roman rhetorical theories are broadly based. When one reads Watson's footnotes thoroughly and carefully, one can see that he has generally avoided the problem identified by Douglas; not all NT scholars who deal with Greek and Latin traditions of rhetoric have done so. A central issue in evaluating

the accuracy of practice of rhetorical criticism is the degree to which a rhetorical critic makes it clear to his/her readers that the critic of necessity must construct a model out of a complicated set of rhetorical traditions (traditions often in very significant tension with each other), in order to make these traditions understandable and usable in criticism; yet that the resulting model is not itself Graeco-Roman rhetoric, only a part thereof. Good criticism of anything always forces us to return to re-examine the sources of our criticism (in this case, rhetorical speeches, rhetorical handbooks, and other Hellenistic school traditions). Watson's dissertation invites those who work carefully through its pages to examine ever more critically the breadth, depth and especially the diversity of rhetorical traditions.

A difference of opinion now exists among rhetorical critics as to how rhetorical *dispositio* (arrangement) relates to the divisions of letters made famous by epistolary form criticism. What is the rhetorical value of the epistolary prescript of a letter? H. D. Betz chose to designate the epistolary prescript as a separate section, before the *exordium*, which implied that the formal rhetoric was inside what he termed an "epistolary frame". The present reviewer, along with Robert Jewett and Wilhelm Wuellner *et alii*, has argued that that the epistolary prescript constitutes the first part of the *exordium* itself, normally followed by a thanksgiving prayer and often a request for intercessory prayer. This designation has been made on the basis of the traditional rhetorical functions of the *exordium* (to introduce the rhetor, his *ethos* and the subjects of the discourse, and to gain the good-will of the audience) which we have identified in the epistolary prescript of letters. Watson has chosen something of a *via media*, in that he acknowledges that the epistolary prescript does function to make "the audience attentive, receptive, and well-disposed; establishing *ethos*, and introducing topics to be developed in the *probatio*" (41). Watson correctly points out that "[t]he epistolary prescript, although necessitated by the epistolary form of the discourse and not technically a recognized element in rhetorical arrangement, does function like the *exordium*" (ibid.). Underlying the seemingly small distinction as to whether the epistolary prescript is part of the *exordium* or a *quasi-exordium* are two most crucial questions that we shall be discussing for years to come: (1) the relationship between epistolary form criticism and rhetorical criticism, and (2) how rhetorical categories are to be applied to letters. Are we to understand that the rhetoric of a letter functions within an epistolary frame, or are we to understand the epistolary form as being utilized by the rhetor as a part of the persuasive text? If we cannot as yet decide the issue, it is enough to identify rhetorical elements and strategies wherever one finds them in letters, and that is what Watson has done in this book.

Of great interest to NT scholars who espouse partition theories of the Corinthian correspondence will be Watson's conclusions about the literary integrity of 2 Peter, pp. 147-159. His statement in these pages as to how the analysis of invention, arrangement and style contribute to the determination of the literary integrity of the text (or its being an edition from original fragments) is very well put. Watson's statement can be seen as a full

statement of what was argued in brief by Robert Jewett in *The Thessalonian Correspondence* (Philadelphia 1986) 34-36, in response to partition theories of 1 and 2 Thessalonians. Watson very reasonably concludes that rhetorical criticism contributes a great deal to the determination of the literary integrity of 2 Peter. On pp. 160-187 Watson argues very evenhandedly in favor of the literary dependence of 2 Peter on Jude, and he concludes that "our examination has shown that rhetorical-redactional analysis yields mixed results in its attempt to determine the direction of the literary dependence between Jude and 2 Peter" (187). Scholars who are interested in determining the direction of literary dependence will take special note of Watson's proposal on pp. 170-171 of what material (in terms of rhetorical arrangement) from an earlier discourse is likely to be taken up and reworked by an editor of a later discourse. Watson argues here that material which came from the *probatio* of Jude (since it would be less situation specific than material from other parts of the discourse) was more likely to be used in 2 Peter than vice versa. Watson's proposal breaks new ground in redaction criticism and, from the standpoint of rhetorical criticism, is quite convincing.

Watson's work, while probably not the last word on the subjects of the literary unity of 2 Peter and the literary dependence of 2 Peter and Jude, follows a set of methods carefully and clearly, and therefore makes a solid contribution towards solving these old problems. This work illustrates the fruitfulness of the current discussion among classical rhetoricians and NT and patristic scholars. This book provides an excellent example for the use of a (relatively) new method, a statement of the study's *raison d'être*, combined with an impressive integration of the new results with the observations of older scholarship; and finally, a clear statement of what has been gained exegetically by the use of the new method. One hopes that similar methodological clarity will be achieved by practitioners of other new methods.

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- C. BONNET, *Melqart. Cultes et mythes de l'Héraclès tyrien en Méditerranée* (Studia Phoenicia VIII). Leuven, Peeters-Presses Universitaires de Namur, 1988. 490 p. 16,5 × 24. 2.950 FB.

L'auteur, déjà connue par quelques études antérieures sur le sujet (cf. les références aux pp. 445-446) a réuni en un splendide volume, y compris du point de vue de la présentation formelle, le fruit de longues années de recherche sur le dieu tyrien Melqart. Ce dernier fut identifié, à partir du Ve siècle av. J.-C. au moins, à Héraclès et appelé par les auteurs classiques l'Hercule tyrien. Il est connu depuis le début des études d'épigraphie phéni-

cienne suite à la publication des deux inscriptions bilingues de Malte CIS I, 122 et 122 bis (= *KAI* 47) et a fait l'objet d'une enquête exhaustive. L'A. a tenu compte, en effet, non seulement des sources directes, bien peu nombreuses — elles ne dépassent pas la dizaine — comme elle le reconnaît explicitement à la p. 5, mais aussi des sources indirectes comme les témoignages classiques, l'iconographie et la numismatique. Ce travail, à propos duquel on a enregistré une appréciation substantielle, en dépit de quelques réserves, de la part de S. Moscati dans *RStFen* 16 (1988) 253-256, suit les attestations de Melqart/Héraclès de Tyr depuis la métropole phénicienne jusque dans tout le bassin méditerranéen, en relation avec l'expansion coloniale phénicienne.

Après la préface, qui présente les objectifs et les méthodes de travail, et une introduction où l'A. étudie la généalogie mythique d'Héraclès/Melqart, le volume se divise en deux parties, suivies d'une conclusion. Dans la première partie, chapitres I-VIII, l'A. examine les témoignages de divers types qui accompagnent la présence de la divinité en question dans le bassin méditerranéen (I: Tyr, 27-113; II: la région syro-palestinienne, 115-144; III: les confins orientaux et occidentaux de la région précédente, 145-163; IV: Carthage et l'Afrique du Nord, 165-201; V: la Péninsule ibérique, 203-241; VI: l'Italie, y compris l'archipel maltais et certaines provinces de l'empire romain, comme la Grande-Bretagne, 311-312, et la Pannonie, 312; VII: Chypre, 313-341; VIII: la Grèce continentale et insulaire, 343-395).

Dans la seconde partie, 399-433, l'A. aborde deux problèmes en deux sections, A et B, respectivement 399-415 et 417-433, consacrées, pour la première, à la question de l'assimilation de Melqart à Héraclès et, pour la seconde, aux antécédents historico-religieux de Melqart dans le cadre de la religion syro-palestinienne antérieure des Phéniciens. Suivent, aux pp. 435-438, les conclusions dans lesquelles l'A. synthétise les résultats de ses recherches. Le volume se clôt par une série de cartes géographiques et une ample documentation iconographique, sans compter les *indices* et la bibliographie.

Les résultats obtenus par l'A. sont particulièrement importants parce que la recherche, menée selon les méthodes les plus récentes des études historico-religieuses, dépasse les conceptions surannées relatives aux Sémites qui avaient inspiré les travaux précédents sur le sujet, comme le concept de *dying god* de Frazer ou celui de triade illustré par R. Dussaud et R. Du Mesnil du Buisson. En outre, dans le sillage de la méthodologie de l'école romaine d'Histoire des religions de R. Pettazzoni et de A. Brelich, l'A. tient compte tant de la structure du panthéon polythéiste que des caractéristiques typologiques de la figure du héros à laquelle Melqart/Héraclès appartient par divers traits. C'est précisément cette dimension héroïque qui a favorisé l'*interpretatio* de Melqart en Héraclès à partir du V^e siècle av. J.-C. au moins, étant donné qu'à cette époque Héraclès est devenu, en milieu grec, le champion de l'hellénisme, le modèle du voyageur et du diffuseur de la civilisation grecque dans le bassin de la Méditerranée. L'A. le montre en se basant sur des recherches antérieures et d'autres publications prochaines, mises à sa disposition par C. Jourdain-Annequin. Dans la seconde partie, l'A. établit la connexion qui existe entre Melqart et le culte des ancêtres royaux divinisés et héroïsés qui constitue une des plus importantes découvertes de l'«école romaine» au sujet de la religiosité de la zone syro-palestinienne.

Cette conclusion nous paraît digne d'être relevée dans le cadre du débat sur la continuité/discontinuité culturelle caractérisant la civilisation phénicienne par rapport aux autres populations de la région palestinienne. Ce point, qui a des répercussions d'ordre linguistique, semble du reste confirmé en Palestine orientale par l'importante activité archéologique qui se déroule dans le Royaume Hachémite de Jordanie (pour le passage de l'Age du Bronze à celui du Fer en Jordanie et son analogie avec la Phénicie, voir les remarques de J. A. Sauer dans les Actes, de prochaine parution, du IV^e Congrès sur l'histoire et l'archéologie en Jordanie, Lyon 30 mai-4 juin 1989). Voir, par exemple, les communications de F. Braemer, *Occupation du sol dans la région de Jerash aux âges du Bronze Récent et du Fer*, et de L. G. Herr, *Shifts in Settlements Patterns of Late Bronze and Iron Ages in the Greater Amman Areas*. En ce qui concerne le point de vue linguistique, on pourra se référer à F. Israel, *Geographic Linguistics and Canaanite Dialects: Current Progress in Afro-Asiatic Linguistics*. Papers of the Third International Hamito-Semitic Congress, London 1984 (Current Issues in Linguistic Theory 28; Amsterdam 1984). Pour une même approche des dialectes cananéens de Palestine, voir R. W. Garr, *Dialect Geography of Syria Palestine, 1000-586 B.C.E.* (Philadelphia 1985) où aux pp. 229, 231, on trouvera le phénicien et l'ammonite, ainsi que le moabite aux extrêmes de la représentation graphique du continuum linguistique.

Après avoir présenté les résultats atteints par l'enquête et leur importance, on doit encore dire que l'A. a su, en utilisant une terminologie chère à M. Sznycer (dont l'A. souligne dans la préface l'apport méthodologique à son enquête) éviter l'écueil classique, en d'autres termes, elle n'a pas voulu retrouver la présence de Melqart/Héraclès partout dans le bassin méditerranéen. Cette attitude méthodologique a amené l'A. à se montrer prudente dans certains cas — à Jamnia (129), à Sardes et à Tarse (153-157) ou en Italie pour certaines représentations provenant de Tharros (fig. 18-19) ou pour la statue de Sciacca (266-267) qui donna lieu à un vif débat dans les années soixante et le début de la décade suivante ou celle de Mozia (274-276) qui a suscité une non moins vive discussion ces dernières années. (Sur la statue de Mozia, voir les évaluations présentées au séminaire de Marsala sur le sujet dans N. Bonacasa-A. Buttitta [a cura di], *La statua marmorea di Mozia e la scultura di stile severo in Sicilia: Atti della giornata di studio, Marsala 1 giugno 1986* [Roma 1988]; en particulier dans ce volume, voir les contributions de A. M. Bisi, *La statua di Mozia nel quadro della scultura fenicio-punica di ispirazione greca*, 69-78 et les conclusions de P. E. Arias, 143-148). Dans d'autres cas, cette attitude a conduit l'A. à formuler des exclusions plus rigides comme dans le cas des éventuelles attestations de Melqart dans le Latium (301, 304-306) soutenues par D. Van Berchem.

Après ce bilan positif, on peut toutefois faire quelques remarques au sujet de ce volume qui, à notre avis, n'en ressort pas du tout diminué. On espère d'ailleurs que des recherches ultérieures, conduites avec une méthodologie semblable sur d'autres divinités phénico-puniques, lui feront suite. Ces remarques visent d'une part à enrichir la bibliographie et d'autre part à signaler des matériaux apparus depuis la publication du volume.

En ce qui concerne les études de Culican signalées à p. 449, celles-ci ont rassemblées dans un volume commode: W. Culican, *Opera Selecta from*

Tyre to Tartessos (Göteborg 1986) 195-264, 281-310, comme c'est aussi le cas des études de numismatique punique de L. Muller signalées à la p. 190, note 123; elles ont été récemment republiées en éditions anastatique par l'éditeur Forni de Bologne.

Voici les autres observations faites au cours de la lecture du volume. P. 54: pour le sceau Vattioni, *Sigilli fenici* n° 25, on peut signaler que la lecture correcte de la dernière ligne fut en premier lieu proposée par F. Benary, *JA* 1844 (2) 310. P. 60: il faut signaler que la même expression *mlqrt bsr* se présente sur un poids du V^e siècle provenant de Tyr et récemment publié par P. Bordreuil, *Syria* 65 (1988) 438, fig. 3 à la p. 439. Pp. 74-77: sur le mythe de la découverte de la pourpre, à côté du matériel utilement rassemblé par l'auteur dans les sources classiques, on pourrait aussi ajouter d'autres témoignages orientaux à ceux déjà recueillis par S. Bochart, comme le passage du célèbre texte de la «Caverne des trésors», pour une édition plus récente que celle de C. Bezoid, voir celle de Su Min Ri CSCO 486, *Scriptores Syriaci*, 282-285. Pp. 78-80: au sujet de l'iconographie de Melqart, on verra à présent le nouveau matériel publié par J. Elayi, «A Phoenician Vase Representing God Milqart», *Baghdader Mitteilungen* 19 (1988) 545-547, même si J. Elayi elle-même précise qu'il s'agit d'une «mere hypothesis».

P. 133, note 96: sur la stèle d'Alep (= *KAI* 201), il faut se ranger à la prudence de l'A. qui renonce à lire la seconde ligne. A la note 96 de la p. 133, on doit ajouter W. T. Pitard, «The Identity of the Bir Hadad of the Melqart Stela», *BASOR* 272 (1988) 3-19, dans lequel à la p. 17, note 2, sont signalées les différentes lectures proposées à ce jour pour la deuxième ligne de la stèle d'Alep; on doit aussi remarquer que, pour ce qui nous concerne, la lecture proposée par ce collègue américain ne nous satisfait pas en raison du manque de parallèles onomastiques. Pp. 136-144: au paragraphe 13, l'A. parle d'«Israël» pour désigner les royaumes de Samarie et de Juda, mais une telle dénomination, malheureusement, n'échappe pas à la critique. Nous aurions préféré que l'A. utilise une autre terminologie, à la lumière de ce qu'a récemment démontré Ahlström à propos du caractère postexilique du terme «Israël» pour désigner l'ensemble des deux royaumes de Samarie et de Juda. P. 152, note 43: sur la statue d'Héraclès, après la présentation provisoire signalée ici, voir maintenant pour l'inscription l'étude de F. Pennacchietti, «L'iscrizione bilingue greco-partica dell'Eracle di Seleucia», *Mesopotamia* 22 (1987) 169-185. P. 263, note 76: sur *bby*, on peut voir à présent, F. Mazza, «B'BY nelle iscrizioni di Antas: dati per una nuova proposta», *RStFen* 16 (1988) 47-56. P. 326, note 143: sur l'inscription de Ialysos, cf. M. G. Amadasi Guzzo, «Iscrizioni semitiche di Nord-Ovest in contesti greci e italici (X-VII) secolo A.C.)», *Dialoghi d'Archeologia* III, 5 (1987) 13-27 et en particulier pour notre inscription les pp. 16-17.

On relèvera enfin les erreurs de typographie suivantes: P. 49, note 89: substituer *supra* à *infra*. P. 105, note 365: renvoyer à la note 50. P. 118, note 16: renvoyer à la note 15. P. 155, note 61: femminilità. P. 243, note 3: renvoyer à la note 1. P. 244, note 6: renvoyer à la note 1. P. 323, note 43: le nom exact de Lagarce est E. et non F. P. 270: figures.

NUNTII PERSONARUM ET RERUM

Quinze lettres du Père Lagrange au Père Lyonnet: 1933-1938 (*)

*Hommage à l'École Biblique
de Jérusalem 1890-1990*

Un demi-siècle ou presque sépare les deux hommes. Marie-Joseph Lagrange, O.P. (1855-1938), achève sa longue carrière, Stanislas Lyonnet, S.J. (1902-1986), la commence. L'initiative de cette correspondance revint au jésuite, mais toutes ses lettres ont été détruites, probablement par L.-H. Vincent, O.P., sur ordre du Père Lagrange. Par contre, les lettres de ce dernier ⁽¹⁾ forment le seul lot que Lyonnet a conservé, toute sa vie durant, d'une abondante correspondance. Signe déjà de l'importance qu'il attachait à ses contacts avec le maître dominicain.

Quand Lagrange reçoit la première lettre de Lyonnet, écrite probablement en juillet 1933, le vieil exégète de Jérusalem vient de publier son *Histoire ancienne du canon du Nouveau Testament* ⁽²⁾. Lyonnet, lui, n'est pas encore prêtre; quelques mois plus tôt, il a obtenu le diplôme de l'École des Hautes Études à Paris avec la publication de sa thèse sur *Le Parfait en Arménien classique, principalement dans les Évangiles et chez Eznik* ⁽³⁾, qui fit de lui un des spécialistes de premier plan de la langue arménienne; pour l'heure, cependant, il vient d'achever sa première année d'études théologiques à Lyon-Fourvière.

(*) N. D. L. R. En publiant ces documents d'archives dans *Biblica*, la Rédaction entend souligner l'importance qu'elle attache à cette correspondance. En révélant la méthode de travail du grand exégète de Jérusalem, dont la compétence est bien connue, ces lettres montrent aussi comment, voici plus d'un demi-siècle, naquit une collaboration bienfaisante entre le fondateur de l'École Biblique et celui qui deviendrait un des maîtres de l'Institut Biblique. Leur publication est pour notre revue l'occasion de rendre hommage à l'École Biblique pour son centenaire.

N. B. *Abréviations*: AGOP: Archives générales de l'Ordre des Prêcheurs, Rome (documents aimablement communiqués par B. Montagnes, O.P., que je remercie ici cordialement); ARSI: Archives romaines de la Compagnie de Jésus; APIBJ: Archives de l'Institut Biblique Pontifical, Jérusalem; APIBR: Archives de l'Institut Biblique Pontifical, Rome; ASEJ: Archives du Couvent de Saint-Étienne, Jérusalem; *Critique textuelle*: M.-J. LAGRANGE avec la collaboration de St. LYONNET, *Introduction à l'étude du Nouveau Testament*. Deuxième partie: *Critique textuelle*. II: *La critique rationnelle* (ÉB; Paris 1935) xvi-685 p.

⁽¹⁾ Trouvées après le décès de St. Lyonnet, elles ont été signalées par A. Vanhoye dans *Acta Pont. Inst. Bibl.*, 9,3 (1987) 167; *Bib* 68 (1987) 141, et dans St. LYONNET, *Études sur l'épître aux Romains* (AnBib 120; Roma 1989) ix. On verra que l'échange de lettres débuta avant la collaboration à *Critique textuelle*.

⁽²⁾ ÉB; Paris 1933.

⁽³⁾ Paris 1933. Le document placé en tête du livre signale que Lyonnet obtint le diplôme le 6 novembre 1932.

Lettre n° 1:

Abey Mont Liban, 12 août 1933

Mon Révérend Père,

Je suis très touché que vous ayez bien voulu destiner votre étude sur la version arménienne à la *Revue Biblique*⁽⁴⁾, et je reconnais dans la première pensée la fidèle amitié des RR. PP. Condamin et Mariès⁽⁵⁾. Votre ms. m'a suivi dans le Liban où je change d'air et où je me repose. Je n'ai donc pu l'étudier, n'ayant ici aucun livre. Je l'ai lu cependant avec soin et intérêt. Ci-joint mes réflexions, d'un amateur tout à fait incompetent en arménien. Vous en ferez l'usage qui vous paraîtra le plus convenable⁽⁶⁾. Le P. Vincent étant seul directeur de la *Revue Biblique*, et en France, je lui envoie le ms. recommandé chez M. Gabalda⁽⁷⁾. Je serais très étonné qu'il n'en fût pas aussi satisfait que moi-même, et très flatté du choix que vous avez fait de la *Revue Biblique*. Le numéro d'octobre est déjà prêt: en dépit des apparences que pourraient suggérer nos retards. J'espère que l'insertion sera possible en janvier, malgré l'inconvénient de verser trop dans la critique textuelle, car la publication des Pap. Chester Beatty ne nous permet pas de différer un très ample compte rendu⁽⁸⁾. Veuillez être assez bon pour excuser ce retard et ne pas nous priver de votre collaboration qui me fait d'autant plus de plaisir que nous sommes tout à fait d'accord. Excusez aussi des remarques tout à fait en l'air, et veuillez agréer mes sentiments respectueux en N. S.

MJ Lagrange
des fr. pr.

(4) Paru sous le titre: «La version arménienne des Évangiles et son modèle grec. L'Évangile selon saint Matthieu», *RB* 43 (1934) 69-87.

(5) Lagrange entretint une correspondance suivie avec A. Condamin, S.J. (1862-1940); quelques soixante-dix lettres de Lagrange sont conservées aux Archives des jésuites de France à Paris. Sur Condamin, cf. R. BROUILLARD, dans *Catholicisme*, II (Paris 1949) 1479. Sur L. Mariès, S.J. (1876-1952), cf. *Mémorial du cinquantenaire de l'École des Langues Orientales Anciennes de l'Institut Catholique de Paris 1914-1964* (Paris 1964) 231-233, et G. DUMÉZIL, dans *RHR* 155 (1959) 268-270.

(6) En marge, Lagrange demande: «Seriez-vous parent du très bon docteur Lyonnet et de son père?». Il pense au médecin de Lyon qui en 1905 soigna L.-H. Vincent: cf. B. MONTAGNES, *Exégèse et obéissance* (ÉB; Paris 1989) 84.

(7) Gabalda est l'éditeur de la *RB* et d'ÉB. Voici la réponse de L.-H. Vincent, du 23 août: «...je reçois du P. Lagrange l'étude que vous avez eu l'amabilité de lui adresser pour la *RB*. Je viens de la lire avec le plus vif intérêt et je m'empresse de vous en faire agréer mes meilleurs remerciements. Elle partira aujourd'hui même pour l'imprimerie. Bien que le n° d'octobre soit assez avancé en composition, je suis si heureux de cette bienveillante collaboration que je veux essayer de remanier le n° d'octobre pour y insérer cette monographie que le Père Lagrange me dit tout à fait décisive. Si pourtant vous n'aviez pas le loisir de revoir en ce moment les épreuves qui vous seront adressées sous peu, ne vous en préoccupez pas et l'article passera au n° de janvier -34...».

(8) Il s'agit de l'article de LAGRANGE, «Les papyrus Chester Beatty sur les Évangiles», *RB* 43 (1934) 4-41.

Notes:

La dépendance du texte Θ est admirablement établie. Mais si on y ajoute la dépendance du ms. Θ, individu déterminé, ne faudrait-il pas dire un mot de l'âge de Θ et de la version arménienne⁽⁹⁾? Nos lecteurs ne sont pas tous tellement au courant, ni moi non plus. Mais je suis très heureux que vous souteniez la version d'après le grec; j'avais adhéré pleinement à cette opinion que j'ai maintenue contre Blake⁽¹⁰⁾.

Syrsin et *cur*⁽¹¹⁾ sont traduits en anglais, je pense d'après Burkitt, excellent, mais non pas infaillible, et enfin le français est plus indiqué. Je suis sûr que vous-même êtes assez sémitisant pour traduire, ou un de vos confrères vous rendrait ce service. Ou enfin renvoyer à Burkitt.

Vous citez Tatien: je pense bien que ce n'est pas d'après Soden⁽¹²⁾, parfaitement inepte sur ce point. Mais enfin il serait bon de dire Tatien arabe, ou d'après l'arménien d'Ephrem etc.

T. R. = *textus receptus*, c'est parfait, mais p. 19⁽¹³⁾ «texte reçu ()» peut faire quelque confusion.

A partir de la p. 13, l'arménien ne figure plus guère en traduction latine ou française. Cela n'a aucun inconvénient en général, car vous êtes parfaitement clair, mais pour Mt. XI,5 cela paraît nécessaire⁽¹⁴⁾.

P. 13: arm = fam Θ contre D et T. R.⁽¹⁵⁾.

P. 17: arm = fam Θ (et non Θ lui-même⁽¹⁶⁾) contre D et T. R.⁽¹⁷⁾. C'est donc T. R. qui fait la différence des deux cas. Et cependant il n'est pas toujours cité dans le second cas. Et une note sur la raison d'une double catégorie ne serait pas de trop⁽¹⁸⁾.

P. 18, il faudrait citer Syrsin⁽¹⁹⁾. Aussi bien je ne vois aucune trace de ⲛ, car l'arménien vient directement de Θ, sauf l'addition d'un pronom nécessaire, puisque le participe était remplacé par un verbe défini. — Je sens le ridicule de cette remarque; ignorant l'arménien, je

⁽⁹⁾ Lyonnet n'a pas répondu à cette suggestion.

⁽¹⁰⁾ Cf. LAGRANGE, «Le groupe dit césaréen des manuscrits des Évangiles», *RB* 38 (1929) spéc. 490-494.

⁽¹¹⁾ Syrsin = version syriaque sinaïtique, *cur*: version syriaque de Cureton: cf. *Critique textuelle*, 203, avec en note le renvoi à F. C. BURKITT, *Evangelion da-Mepharreshe* (Cambridge 1904). Lyonnet a donc traduit en français les citations de syrsin et *cur*: cf. ses pp. 74 ss.

⁽¹²⁾ H. F. von Soden fut critiqué pour son traitement de Tatien par LAGRANGE, «Une nouvelle édition du Nouveau Testament», *RB* 22 (1913) spéc. 510. Lyonnet citera de fait Tatien arabe à ses pp. 76, 80 et 85. Sur le texte et le commentaire d'Ephrem, cf. *Critique textuelle*, 192-193.

⁽¹³⁾ A la p. 19 du manuscrit (perdu) de Lyonnet, correspond, semble-t-il, la p. 85 de l'article de la *RB*; Lyonnet aura corrigé.

⁽¹⁴⁾ Cf. *RB* 43 (1934) 80.

⁽¹⁵⁾ Sous-titre à cette même p. 80.

⁽¹⁶⁾ En marge, avec renvoi, Lagrange ajoute: «donc comme p. 13».

⁽¹⁷⁾ Cf. le sous-titre à la p. 83, mais Lyonnet y a omis «et T. R.».

⁽¹⁸⁾ Lyonnet ajouta cette note: *RB* 43 (1934) 84, n. 1.

⁽¹⁹⁾ A la p. 84 de l'article publié.

m'expose à votre sourire: *ne sutor ultra crepidam*⁽²⁰⁾. Soyez indulgent. Si vous aviez quelque changement à faire, vous le feriez sur l'épreuve. Il va sans dire que je n'en *demande* aucun!

Dans le fascicule d'octobre 1933 de la *RB*, donc après le premier échange de lettres entre Lyonnet et Lagrange, celui-ci publie son «Projet de critique textuelle rationnelle du Nouveau Testament»⁽²¹⁾. Durant les mois qui viennent, Lagrange doit travailler avec acharnement à sa réalisation puisque le gros volume, *Introduction à l'étude du Nouveau Testament*, II: *Critique textuelle*, 2: *La critique rationnelle*, paraîtra en octobre 1935.

Lettre n° 2:

Jérusalem, 3 mars 1934

Mon Révérend Père,

L'amabilité que vous avez eue de nous envoyer un bel article pour la *Revue Biblique* encourage mon indiscrétion. Je voudrais vous demander un service beaucoup plus considérable. Voici le fait, en toute simplicité. Je travaille à un manuel de critique textuelle du N. T. qui formera un assez gros volume. Mais je suis tout à fait incompetent pour l'arménien et le géorgien. Auriez-vous la très grande obligeance de vous charger de cette partie, en nom propre, comme collaborateur pour cette partie?

Sur l'arménien, je ne saurais admettre la thèse encore soutenue par M. R. Blake d'une traduction du syriaque⁽²²⁾. Mais ce n'est pas non plus votre pensée. Pour le reste, le peu que je vois d'après votre article me fait penser que nous serions facilement d'accord. D'après mon plan, il y a une section pour l'évangile, où sont traitées les questions générales pour les versions; puis pour les Actes, s. Paul, les Catholiques, l'Apocalypse, on donne les détails spéciaux, s'il y a lieu. Il ne faudrait guère, à mon goût, qu'une cinquantaine de pages pour l'arménien, environ vingt pour le géorgien. Vous auriez soin de [les] faire examiner par vos censeurs: en matière de critique textuelle, je ne crois pas qu'il y ait de difficulté⁽²³⁾. Pour chaque version, je traite §1. Documents. §2. Caractère de la version, une ou multiple. §3. Origine de la version et ses destinées. Le caractère [est] marqué par quelques exemples bien choisis; aucune prétention à donner des listes complètes. Je ne puis entreprendre que tout soit *gebucht*, à la façon de von Soden.

⁽²⁰⁾ Proverbe cité par Plinie, *Hist. Natur.* 35, 36, 85: «ne supra crepidam sutor».

⁽²¹⁾ *RB* 42 (1933) 481-498.

⁽²²⁾ Cf. LAGRANGE, «Le groupe dit césaréen», spéc. 489-493, renvoyant à K. LAKE, R. P. BLAKE et S. NEW, «The Caesarean Text of the Gospel of Mark», *HTR* 21 (1928) spéc. 307-312 et 324-325.

⁽²³⁾ Lagrange redoute, semble-t-il, de nouvelles difficultés avec la Compagnie de Jésus. Le 14 mars 1934, il écrit à Condamin, à propos de sa requête adressée à Lyonnet: «De plus je ne voudrais pas le compromettre, et cette crainte a augmenté ces jours-ci» (Archives des jésuites de France, Paris; texte transmis aimablement par B. Montagnes, O.P.).

Il serait bien entendu que vous accepteriez un honoraire convenable, soit en valeur fixe, soit en prenant un certain nombre d'exemplaires gratuits.

J'ai quelque espérance — *vita comite*⁽²⁴⁾ — d'avoir terminé en juillet, surtout si vous m'ôtez cette épine du pied. Je vous serais donc très obligé de m'envoyer le ms. au début juin. Mais vous n'avez sans doute pas que cela à faire.

Si vous ne pouviez m'accorder cette faveur, je vous serais obligé de regarder ma démarche comme confidentielle. Dans le cas contraire, que j'espère, il est bien évident que vous aurez à prévenir vos supérieurs pour avoir leur agrément.

Veillez, mon Révérend Père, me rappeler au bon souvenir du T. R. P. Condamin, mon vieil ami: il est de ceux qui savent concilier l'affection avec le franc-parler, et j'espère qu'il ne m'en aura pas voulu de mon obstination à ne pas me rendre *entièrement* à son système⁽²⁵⁾.

Veillez agréer aussi mes sentiments respectueux en N. S. et de nouveau mes remerciements pour votre bel article,

f. MJ Lagrange
des fr. pr.

Comme le lui suggérait en toute droiture le Père Lagrange, Lyonnet dut contacter sans tarder son supérieur provincial, Christophe de Bonneville, S.J., qui lui expédia, dès le 15 mars, d'Yzeure (Allier), le billet suivant: «Mon cher Père, P. X., Vous pouvez faire le travail qui vous est demandé. Je ne me rends pas assez compte de sa nature pour voir s'il y aura lieu de le soumettre au censeur romain. En tout cas je ne vois pas que l'agrément préalable, et, pour ainsi dire, de principe, soit requis. En union de prières etc. »⁽²⁶⁾.

Lyonnet qui, entre temps, avait donné une première réponse à Lagrange, lui communique, le 28 mars, l'accord de son supérieur. Sur ce, Lagrange reprend la plume.

Lettre n° 3:

Jérusalem, 2 avril 1934

Mon Révérend Père,

Je reçois votre lettre du 28 mars, et j'avais reçu la précédente en son temps. Je m'excuse de n'avoir pas répondu aussitôt; j'attendais ce qui me paraissait l'essentiel, la réponse du T. R. P. Provincial. Je vous prie de le remercier très vivement de son agrément.

Et je vous remercie vous-même d'avoir pris la chose si à cœur. En somme, le mois d'août ne serait pas trop tard, ni même le mois de septembre. J'ai encore bien à faire.

⁽²⁴⁾ Gn 18,10 Vulg.

⁽²⁵⁾ Allusion à son livre *Poèmes de la Bible* (Paris 1933) que Lagrange recensa dans *RB* 43 (1934) 128-132.

⁽²⁶⁾ APIBR.

Je suis heureux de vos découvertes. M. Blake m'a été d'abord recommandé comme hôte par le R. P. P. Peeters⁽²⁷⁾, l'illustre Bollandiste. Il est très agréable, mais j'avais déjà contesté son opinion dans mon étude sur le groupe césaréen⁽²⁸⁾. Vous êtes à même de le faire beaucoup mieux. Vous savez sans doute que Madame New est devenue Mrs. Lake⁽²⁹⁾.

Je ne prétends pas m'ingérer dans votre travail. Si vous n'y voyez pas d'inconvénient, vous pourriez mettre l'étude principale à propos des évangiles: les documents, mss et imprimés, le caractère de la version, son origine, sa date, le texte base qu'elle suppose. Cela une fois dit, vous ne traitez, pour les Actes, les Ep. paulines, les Catholiques, l'Apocalypse, que les spécialités. Pour l'Apocalypse, je ne vois pas trace d'un texte *D* (Soden I, après n'avoir parlé dans ses prolégomènes que d'un texte *Av* [André de Cappadoce!])⁽³⁰⁾. Un seul texte à l'origine et longtemps, suivi par les latins et la syrienne de Gwynn (que je ne crois pas philoxénienne), puis évolution du texte ecclésiastique. Nous savons par Ch. Beatty que les corrections élégantes ont commencé de très bonne heure, du moins je crois l'avoir montré dans mon article de *RB* [en] janvier⁽³¹⁾. J'attends avec impatience la suite...

Merci encore, mon cher Révérend Père, et veuillez croire à mes sentiments respectueux en Christo Jesu. Mon affectueux souvenir au bon Père Condamin,

fMJ Lagrange
des pr.

Le 12 mai, Lagrange écrivait au T. R. P. M. Gillet, Maître Général des Frères Prêcheurs: «J'achève ces jours-ci un volume de bien 500 pages in-8° sur la critique textuelle du Nouveau Testament. Pour l'arménien et le géorgien, j'ai demandé la collaboration du R. P. Lyonnet, S.J., qui me l'a accordée avec le plus grand empressement, et son provincial aussi»⁽³²⁾. Tandis que Lyonnet est au travail de son côté, Lagrange rentre en France à la fin du mois de juin et jusqu'au 12 septembre.

⁽²⁷⁾ Lagrange se trompe plus d'une fois dans l'orthographe des noms propres (ici il écrit «Peters»); dans la présente édition, j'ai clarifié aussi la ponctuation. Sur le Père Peeters, jésuite belge, cf. P. DEVOS, «Le R. P. Paul Peeters (1870-1950)», *AnBoll* 69 (1951) I-XLVII. Peeters et Lagrange s'appréciaient depuis une trentaine d'années.

⁽²⁸⁾ Lagrange a donné son avis sur la position de Blake dans «Le groupe dit césaréen», spéc. 488-494.

⁽²⁹⁾ Cf. *supra*, n. 22.

⁽³⁰⁾ Cf. VON SODEN, *Die Schriften des Neuen Testaments*, I,4 (Berlin 1910) 2051-2067 («Av-Text»), critiqué par Lagrange dans *Critique textuelle*, 579 et 621.

⁽³¹⁾ Cf. *supra*, n. 8. En marge de la première page de sa lettre, Lagrange ajoute: «Si je vais en France, je ne manquerai pas de vous donner rendez-vous; ce sera le meilleur moyen de tout arranger».

⁽³²⁾ AGOP.

Lettre n° 4:

Culoz, Ain, 14 août 1934

Mon Révérend Père,

Je pense que vous avez maintenant enfin reçu la table des matières de l'ouvrage auquel vous voulez bien collaborer et que vous n'aurez pas été troublé par les fautes d'impression. Il vous est donc loisible d'y insérer la partition des chapitres qui vous sont réservés. Si la version arménienne était sans aucune importance sur un point, par exemple les épîtres catholiques, on pourrait supprimer ce chapitre, mais je ne le crois pas. Je dis arménienne, et aussi géorgienne.

Je désirerais bien vous revoir. Je pense, s'il plaît à Dieu, être lundi 20 chez Mademoiselle Falsan à St Cyr-au-Mont-d'Or (Rhône)⁽³³⁾. Je vous prierais de m'y écrire un mot pour me dire si je puis vous trouver rue St^e-Hélène⁽³⁴⁾ le mardi, le mercredi ou le jeudi et de préférence dans l'après-midi. Si vous préféreriez monter à St Cyr, ma cousine serait très honorée de vous recevoir. C'est un peu difficile à trouver, non pas au gros du village, mais au hameau de La Chaux, au sud du grand château Perret.

J'espère que votre travail est déjà avancé: nous pourrions en causer. Mais si vous êtes absent de Lyon, ne revenez pas pour cela; nous pourrions nous entendre par lettres.

Veuillez me rappeler au bon souvenir du R. P. Condamin, et agréer, mon Révérend Père, l'expression de mon respect en N. S.

fr. M.-J. Lagrange
des fr. pr.

A cette lettre, Lyonnet répond en annonçant son ordination sacerdotale, le 24 août, à Lyon.

Lettre n° 5:

Chez M^{lle} Falsan, à St Cyr-au-Mont-d'Or,
21 août 1934

Mon Révérend Père,

J'ai trouvé ici votre bonne lettre hier au soir. Je suis extrêmement touché de votre invitation et je vous prie d'en remercier de ma part le T. R. P. Recteur⁽³⁵⁾. C'eût été une consolation pour moi d'assister à votre ordination, mais je dois partir pour Vienne précisément ce matin-là, ayant un double rendez-vous que je ne puis changer, de-

⁽³³⁾ La maison appartenait à la famille Falsan depuis le début du XIX^e s. Mlle Falsan était la fille de Pierre Falsan, le géologue qui découvrit les traces d'un glacier dans la vallée du Rhône; il était le demi-frère de la mère de Lagrange, Élisabeth Falsan.

⁽³⁴⁾ Adresse du collège des jésuites à Lyon.

⁽³⁵⁾ Le recteur du théologat des jésuites à Lyon-Fourvière était alors Charles Chamussy, S.J. (1890-1974).

vant arriver à Roybon⁽³⁶⁾ avec ma sœur qui vient de l'est, et que je n'ai pas encore vue. Mais je ne manquerai pas de m'unir à vos prières, et je vous prie de me comprendre parmi ceux auxquels vous obtiendrez des grâces en ce moment si solennel. Il est bien évident aussi que ce jour sacré n'est pas propre à un entretien d'études, même religieuses. Il me semble d'ailleurs que tout est en bonne voie. J'aurais voulu qu'on vous eût envoyé la table des abréviations, afin que vous puissiez la suivre. Mais je ne l'ai pas encore reçue moi-même. Gabalda ne se presse pas. Une certaine identité me paraît désirable dans une collection, à plus forte raison dans le même volume, par exemple écrire Jo et non pas Jn. Mais je suis le premier à donner en cela le mauvais exemple⁽³⁷⁾.

A partir du 24, vous pourriez m'écrire à Roybon (Isère), chez M. Rambaud, jusqu'au 7 sept. Je dois m'embarquer le 14: je pourrais donc avant cette date recevoir une lettre à Marseille, 35 rue Edmond Rostand⁽³⁸⁾.

Je suis très heureux que vous ayez l'appui du R. P. Mariès: je pense qu'il va bien maintenant et s'entretient avec la Sagesse⁽³⁹⁾.

Veillez, mon Révérend Père, m'accorder de loin une de vos premières bénédictions, et agréer l'expression de mon respect in Christo Jesu

fMJ. Lagrange
des fr pr.

Lettre n° 6:

Roybon, Isère, 6 sept. 1934, chez M. Rambaud

Mon Révérend Père,

Je n'avais donc pas tort d'espérer que vous me feriez une part dans vos premières messes. Soyez remercié, et veuillez me continuer vos bonnes prières.

J'allais vous écrire, quand j'ai reçu votre lettre du 5. Les épreuves sont hérissées de fautes. Vous aurez corrigé et mis σ⁽⁴⁰⁾ pour le texte reçu etc. Je n'ose imposer à M. Gabalda de vous envoyer toutes les épreuves s'il n'a pas continué comme je l'en avais prié. S'il a continué, vous seriez bien bon de l'en dispenser quand vous jugerez que vous en avez assez.

Pour la table des matières, je vous avais à tout hasard — ne sachant rien — réservé 5 chapitres⁽⁴¹⁾. D'après ce que vous me dites, il suffira de trois ou même de deux. Voyez vous-même, pour l'arménien

⁽³⁶⁾ A Roybon (Isère), maison de M. Rambaud, époux de Pauline Lagrange, sœur du Dominicain; cf. LAGRANGE, *Au service de la Bible. Souvenirs personnels* (ed. P. BENOIT) (Paris 1967) 213 et 233. Cf. encore *infra*, Lettre n° 14.

⁽³⁷⁾ Même aveu dans *Critique textuelle*, XII, n. 1.

⁽³⁸⁾ Couvent des Dominicains.

⁽³⁹⁾ Lagrange attendait de Mariès un commentaire de Sg pour ÉB. En fait, ce fut C. Larcher, O.P., qui le fit (ÉB, n. s. 1, 3 et 5; Paris 1983-1985) 1110p.

⁽⁴⁰⁾ En fait: σ pour Stephanus (R. Estienne et son édition du Nouveau Testament de 1550): cf. *Critique textuelle*, XIII et 1; voir, par ex., à la p. 367 (Lyonnet).

⁽⁴¹⁾ Finalement, dans *Critique textuelle*, Lyonnet donna cinq chapitres.

et pour le géorgien. Je vous prie seulement de corriger dans ce sens *toute la table*, et de rectifier d'après votre décision tous les numéros de *tous* les chapitres.

Quant à la «3^e aux Corinthiens», j'en dis moi-même quelques mots, pour noter son existence, mais sans insister⁽⁴²⁾.

J'ai suivi en somme la numérotation du R. P. Merk, pour tâcher d'aboutir à un accord⁽⁴³⁾.

Je pourrais avoir un mot de vous à Marseille, 35 rue Edmond Rostand, avant mon départ, qui aura lieu non plus le 14, mais le 12 sept. avant midi.

Veuillez dire au R. P. Mariès combien je suis heureux qu'il soit complètement remis. La conversation de la Sagesse achèvera sa guérison.

On m'a dit que le futur Recteur de l'Inst. pontif. S.J. de Jérusalem serait le R. P. Lobignac⁽⁴⁴⁾. Nous en serions tous très heureux.

Veuillez, mon Révérend Père, agréer l'expression de mon respect en N. S.

fMJ Lagrange
des fr. pr.

A la fin septembre, semble-t-il, Lyonnet a terminé la rédaction du chapitre concernant les versions arménienne et géorgienne des évangiles, soit 45 pp. du volume imprimé; son manuscrit, perdu, de 66 ou 67 pp., est revu par L. Mariès et surtout par F. Zorell qui, de l'Institut Biblique de Rome, lui fait parvenir plusieurs pages d'observations précises et bienveillantes, dont Lyonnet tiendra compte⁽⁴⁵⁾. Le 17 octobre, il écrit à Lagrange; mais celui-ci ne lira le chapitre de son collaborateur qu'au tournant des années 1934-1935, car Lyonnet dut l'envoyer directement à Paris, à Gabalda, pour l'impression.

Lettre n° 7:

Jérusalem, 26 oct. 1934

Mon cher Révérend Père,

Merci de m'avoir envoyé votre lettre du 17 par avion, mais cela n'avance à rien, car elle n'est arrivée que le 24 au soir. Je suis très re-

⁽⁴²⁾ Seule mention de cette 3^e Corinthiens à la page 517 de *Critique textuelle*.

⁽⁴³⁾ De quelle numérotation s'agit-il? Serait-ce celle des mss, où il y a «en somme» identité entre LAGRANGE, *Critique textuelle*, XIII-XVI et A. MERK, *Novum Testamentum graece et latine* (Roma 1933) 18*-27*?

⁽⁴⁴⁾ Marcel Lobignac, S.J. (1893-1965), fut Directeur de l'Institut Biblique Pontifical de Jérusalem et supérieur de la communauté des jésuites de 1934 à 1939. Cf. *infra*, Lettre n° 7.

⁽⁴⁵⁾ Cf. *Critique textuelle*, 342, n. 1. Lyonnet avait conservé les notes de F. Zorell, rédigées en français: «Notae nonnullae, ad liberum usum auctoris. 1 Thess. 5,21» (4 pp. sur le manuscrit de Lyonnet concernant les évangiles), «Nota ad hoc manuscriptum» (2 pp. sur la partie concernant les Actes), enfin un billet où Zorell se corrige et conclut ainsi: «Je recommande aussi mes autres remarques à votre œil critique, afin que vous n'écriviez pas, séduit par moi, quelque chose qui attire la critique des recenseurs» (APIBR). Le 20 janvier 1936, écrivant à A. Bea, Lyonnet salue «le R. P. Zorell dont la bienveillante revision m'a permis de corriger tant de points défectueux» (APIBR).

connaissant à vos supérieurs d'avoir exécuté si vite la révision et de l'avoir faite cependant avec soin, de façon qu'elle vous fût utile: cas très rare. Donc vous n'êtes pas en retard, puisqu'on avait attendu pour imprimer le ch. V⁽⁴⁶⁾. Quant aux Actes, il sera facile de les insérer dans la seconde épreuve. Si j'ai bien compris, vous bouclerez à cet endroit⁽⁴⁷⁾, car il ne serait pas indiqué de faire des chapitres spéciaux s'il n'y a pas matière. Enfin vous donnerez vos instructions à M. Galbada sur ce point, pour qu'il puisse faire une table des matières conforme au texte.

Naturellement on m'enverra une épreuve de vos chapitres, mais je suis bien sûr d'avance que je n'aurai qu'à vous féliciter. Ce genre de travail n'est d'ailleurs pas le mien, car je suis incapable de suivre une règle fixe dans les sigles, les points, les italiques⁽⁴⁸⁾. Et maintenant je n'ose tout refondre pour ne pas faire trop de frais.

Enfin je n'ai voulu lancer que des idées et un critère, sans être complet. Il me reste à faire une petite conclusion pratique⁽⁴⁹⁾. J'ai eu l'honneur de déjeuner à l'Institut Biblique, ce que j'ai fait par sympathie pour le R.P. Lobignac. Le R^{me} Père Bea a été très aimable⁽⁵⁰⁾. Veuillez me rappeler au bon souvenir du cher Père Condamin et agréer l'expression de ma reconnaissance et de mon respect in Christo Jesu,

frMJ Lagrange
des pr.

Lettre n° 8:

Jérusalem, 23 nov. 1934

Mon cher Révérend Père,

Je suis très touché de votre empressement, de la peine que vous avez prise, et je ne voudrais pas trop dépasser les bornes de la discrétion. Mais puisqu'il ne manque à votre travail que les épîtres catholiques, il me semble qu'il serait bien à propos, pour l'harmonie de l'ensemble, de leur consacrer, fût-ce seulement 2 ou 3 pages⁽⁵¹⁾. Il serait encore temps, car l'impression s'est arrêtée avant la fin de ce qui les

⁽⁴⁶⁾ Dans *Critique textuelle*, le ch. V aborde, à propos des évangiles, «Le texte grec de quelques Pères anciens» (169-181). La première contribution de Lyonnet n'arrive qu'au ch. X: «Les versions arménienne et géorgienne» (des évangiles: 342-386).

⁽⁴⁷⁾ Cf. *supra*, n. 41.

⁽⁴⁸⁾ Cf. *supra*, n. 37. Le correcteur principal des épreuves fut P. Benoit: *Critique textuelle*, IX.

⁽⁴⁹⁾ *Critique textuelle* n'inclut pas cette «conclusion pratique». Faut-il en trouver l'essentiel dans M.-J. LAGRANGE, «La critique textuelle du Nouveau Testament», dans *Initiation biblique* (edd. A. ROBERT-A. TRICOT) (Paris 1939) 236-248?

⁽⁵⁰⁾ On lit dans le diaire de l'Institut Biblique Pontifical de Jérusalem: «Sept. Dom. 30, 1934: Festum S. Hieronymi. Veniunt invitati ad prandium: R.P. Lagrange, RR. PP. Médebielle, Superior Betharram, Superior N.-D. de France, et Bonaventura Ubach, Superior Benedictorum Montserrat. Celebratur hoc die novus Superior, R.P. Lobignac». Le même diaire précise qu'A. Bea, S.J. (1881-1968), recteur de l'Institut depuis 1930, était arrivé à Jérusalem le soir du jeudi 20 septembre: c'était son premier voyage au Proche-Orient (APIBJ).

⁽⁵¹⁾ Ce que fit Lyonnet: *Critique textuelle*, 575-578.

regarde. Je vous envoie toutes les épreuves que j'ai, sans avoir le courage de les corriger; je suis un peu excédé de travail. Vous verrez qu'en particulier je cite la version arménienne pour une très bonne leçon du ms. B⁽⁵²⁾. Je n'ai pas insisté pour vous faire envoyer toutes les épreuves, car j'avais honte que vous les vissiez dans cet état. Mais si vous vouliez être assez bon pour lire et corriger la 2^e épreuve en pages, je vous la ferais envoyer par M. Gabalda. Je deviens bien encombrant, mais tout à fait disposé non seulement à le reconnaître, mais à retirer mes propositions si elles ne vous agréent pas.

Mon affectueux respect, s.-v.-p., au R. P. Condamin, et veuillez agréer mes plus sensibles remerciements et mon respect en N. S.

fMJ Lagrange,
des fr. pr.

Lettre n° 9:

Jérusalem, 4 janv. 1935

Mon cher Révérend Père,

J'attendais pour vous répondre d'avoir reçu épreuves de votre premier chapitre: le Bottin a tout arrêté⁽⁵³⁾. Mais je ne m'attendais pas à recevoir en même temps le manuscrit! Et probablement vous n'avez pas reçu d'épreuves! Je ne suis pas du tout compétent, je ne puis donc corriger les épreuves. Je vous renvoie le tout, même une fin de chapitre que j'ai corrigée, vous priant de transmettre le tout à M. Gabalda. En même temps je lui écris de vous envoyer désormais vos manuscrits et les placards, même s'ils contenaient autre chose au commencement ou à la fin.

Naturellement vous ne seriez chargé que de la correction de votre texte. J'enverrai mes corrections de mon côté. Vous pourriez lui confirmer cet arrangement. J'ai cependant lu votre travail, et j'ai admiré comment vous êtes entré si parfaitement dans la disposition et dans l'esprit du corps du livre. On ne s'apercevra de la différence qu'à la perfection de votre rédaction⁽⁵⁴⁾. Je vous suis très reconnaissant de ce que vous avez fait. Pour la 2^e épreuve, je vous remercie aussi très cordialement d'avoir accepté de la corriger, mais elle est encore en trop mauvais état. Et je pense envoyer la 3^e épreuve à M. Devreesse⁽⁵⁵⁾. Le R. P. Lobignac qui va quitter Jérusalem⁽⁵⁶⁾ pourra vous donner de nos nouvelles. Je vous prie d'accepter pour vous-même et de transmettre à mon excellent et vieil ami le R. P. Condamin mes meilleurs vœux de bonne année, avec mon respect in Christo Jesu,

fMJ Lagrange
des fr. pr.

⁽⁵²⁾ Pour Rm 8,11, dans *Critique textuelle*, 466, et, pour 2 P 2,15, 561.

⁽⁵³⁾ Le Bottin était imprimé chez Firmin-Didot, comme *Critique textuelle*.

⁽⁵⁴⁾ Lagrange ne se cachait pas certaines faiblesses de son style: cf. son livre *M. Loisy et le modernisme* (Paris 1932) 5.

⁽⁵⁵⁾ R. Devreesse collabora au projet: cf. *Critique textuelle*, VII et IX.

⁽⁵⁶⁾ Pour un séjour de quelques mois en France.

Le 31 janvier 1935, au milieu de sa troisième année de théologie, Lyonnet écrit à son Provincial sur les perspectives d'avenir. Il lui soumet trois hypothèses: 1) commencer à l'automne son Troisième An, année de spiritualité que les jésuites accomplissent après leurs études; 2) aller à Beyrouth et y préparer l'édition critique des évangiles arméniens: «Car Beyrouth est tout à fait désignée pour mener à bien cette œuvre, en particulier grâce à la proximité de Jérusalem et du Père Lagrange. Forcément il faudra y prévoir un séjour d'un ou deux mois(...). Attendre, c'est(...) risquer de ne plus pouvoir profiter du Père Lagrange»; ce travail, joint à sa collaboration à la *Critique textuelle* de Lagrange, pourrait peut-être servir de thèse de doctorat en théologie; or, l'année précédente, le Provincial avait suggéré une thèse doctorale en théologie; 3) aller à Paris «qui offrirait des facilités pour le doctorat [—Lyonnet songeait à une thèse sur Irénée—], sinon pour l'édition des Évangiles [arméniens]. Mais ce serait renoncer à l'étude de l'arménien parlé (ou presque) et aux conseils 'uniques' du P. Lagrange»⁽⁵⁷⁾. Finalement c'est la première hypothèse qui fut retenue. De tous ces projets, Lyonnet ne semble pas avoir parlé au P. Lagrange. Celui-ci fête, le 7 mars 1935, ses quatre-vingts ans.

Lettre n° 10:

Jérusalem, 18 mars 1935

Mon Révérend et cher Père,

Pour les titres courants, mettez-les comme vous jugerez le mieux, mais si vous distinguez pour les évangiles, il faudra le faire tout le temps. Le principe que j'avais adopté était de changer les titres courants avec les chapitres. On aurait pu faire deux chapitres, arménien et géorgien, pour les évangiles; mais alors pour le reste? Enfin faites pour le mieux.

Dans la préface et sur la couverture, je mentionne votre aimable collaboration. Mais ne faudrait-il pas mettre: les versions arménienne et géorgienne par le R. P. St. Lyonnet, pour bien signaler votre part? A tout le moins, il me semble qu'il serait bon que votre première note pour les évangiles fut signée: (St. Lyonnet), ou quelque chose de semblable⁽⁵⁸⁾.

Pour l'Apocalypse, Vogels a facilité l'étude des versions latines et je me suis appuyé sur lui, ses observations, veux-je dire, plutôt que ses théories⁽⁵⁹⁾. Mais il m'est bien difficile de vous donner une opinion sur l'arménien.

Le R. P. Condamin, mon vieil ami, et le R. P. Lobignac ont bien voulu m'écrire les lettres les plus aimables⁽⁶⁰⁾, mais je suis vraiment tellement accablé, et à la veille d'un départ pour l'Égypte⁽⁶¹⁾, que je

⁽⁵⁷⁾ APIBR.

⁽⁵⁸⁾ Cf. *Critique textuelle*, titre et 342, n. 1.

⁽⁵⁹⁾ Cf. *Critique textuelle*, 598.

⁽⁶⁰⁾ Lettres perdues qui certes félicitaient Lagrange pour son quatre-vingtième anniversaire.

⁽⁶¹⁾ Il donna au Caire une conférence publiée: «L'Apothéose des Souverains et la divinité du Christ», *Cahiers du Cercle Thomiste du Caire* 2 (1935) 84-97.

ne puis, à mon grand regret, leur exprimer ma reconnaissance. Plutôt que de le faire officiellement par de simples cartes, je préfère vous prier d'être mon interprète auprès d'eux.

Veuillez agréer, mon Révérend et cher Père, mes sentiments respectueux et mes remerciements très cordiaux,

fMJ Lagrange
des fr. pr.

Lettre n° 11:

Jérusalem, 14 juin 1935

Cher Révérend Père,

Un mot seulement, parce que je me remets lentement d'une forte grippe....

J'ai été très touché des bonnes paroles du R.P. Bonsirven, si loyales, tout en étant trop flatteuses⁽⁶²⁾.

Rien de plus aisé que de vous faire des tirages à part. Vous voudrez bien vous entendre avec M. Gabalda pour l'organisation, la couverture etc. Je vous prie de vouloir bien accepter pour cela 1000 francs qui seront avancés par M. Gabalda....: je ne me rends pas bien compte, mais je serais heureux d'ajouter ce qui serait opportun à cette somme une fois fixée.

Vous voudrez bien me dire combien vous désirez d'exemplaires pour vous de tout le manuel.

Nous n'en sortons pas, je ne sais plus où nous allons!

Veuillez agréer, cher Révérend Père, l'expression de ma gratitude et de mon respect,

fMJ Lagrange
des fr. pr.

Alors que le volume de *Critique textuelle* sort de presse, la santé de Lagrange se détériore; le sacrifice est inévitable: au début de l'automne, il quitte Jérusalem! Entre temps, l'avenir de Lyonnet se dessine définitivement, mais il n'en dit rien encore à Lagrange, sauf qu'il a commencé son Troisième An à Paray-le-Monial, le 20 septembre 1935.

Lettre n° 12:

St-Maximin (Var), École de théologie,
5 janv. 1936

Mon Révérend et cher Père,

Votre lettre du 18 déc. m'est arrivée à St-Maximin où je suis depuis le 12 oct.⁽⁶³⁾. Sur un avis fortement motivé des médecins, le

⁽⁶²⁾ Il s'agit de l'article de J. BONSIIVEN, «Le jubilé du R.P. Lagrange», *RSR* 25 (1935) 344-357. Sur Bonsirven (1880-1958), cf. LYONNET, dans *Bib* 39 (1958) 262-268.

⁽⁶³⁾ C'est au couvent des Dominicains de St-Maximin que Lagrange avait commencé sa vie religieuse en 1879. Lyonnet, qui semble n'avoir encore rien su de ce retour, aura envoyé sa lettre à Jérusalem.

P. Général m'a assigné en France. J'ai donc quitté la Palestine, non sans un grand déchirement; mais je ne dois plus songer qu'à la patrie éternelle. Je vous remercie de vos bonnes paroles et de vos vœux, mais c'est moi qui vous suis très reconnaissant pour le concours que vous m'avez si obligeamment donné.

Je recevrai bien volontiers, sans pouvoir l'apprécier, votre échantillon d'une édition du texte arménien de s. Marc⁽⁶⁴⁾. Et il me semble qu'il serait très opportun de donner de cette façon tout le N. T. ou du moins les évangiles. Mais, comme vous le dites très bien, un appareil grec du texte grec serait un peu hors de saison. La traduction (reconstitution) en grec et même en français sera au contraire très utile, avec quelques notes justifiant votre texte grec dans les cas difficiles ou douteux.

Vous êtes bien heureux de faire votre troisième an à Paray-le-Monial. J'ai eu bien souvent envie d'y retourner! Votre présence serait un attrait de plus, car je pense que vous n'y êtes pas dans une solitude absolue. Veuillez prier pour moi Sainte Marguerite-Marie⁽⁶⁵⁾, et agréer mes vœux pour cette précieuse année, avec mon respect en N. S.,

fMJ. Lagrange
des fr. pr.

Sur ces entrefaites, donc, l'avenir de Lyonnet prenait une nouvelle orientation. Le 27 juin 1935, le T. R. P. Wl. Ledóchowski, Général de la Compagnie de Jésus, écrivait au P. Ch. de Bonneville, supérieur provincial de Lyonnet: «Au cours de votre visite à Rome en septembre dernier, j'avais approuvé la spécialisation du P. St. Lyonnet dans l'étude de l'Arménien et une lettre du 20 septembre a confirmé cette approbation. Les circonstances m'obligent à revenir sur cette décision. Pour des raisons graves que je ne puis vous exposer ici, nous avons besoin d'avoir au plus tôt des Docteurs en Écriture Sainte éminents et très sûrs, comme ce serait le cas du P. Lyonnet, qu'il faudrait avertir tout de suite de ce changement d'orientation que je regrette, mais qui est nécessaire»⁽⁶⁶⁾.

Le 22 août 1935, de Lyon où il se trouve encore, Lyonnet envoie à son Provincial un rapport sur sa préparation dans le domaine linguistique et les perspectives qu'elle offre. Ce rapport est envoyé à Rome au Père Wl. Ledóchowski, qui confirme et précise que Lyonnet doit aller à l'Institut Biblique Pontifical de Rome et se préparer à y occuper une chaire de Nouveau Testament à la Faculté Biblique; il ajoutait tout de même que Lyonnet, une fois au Biblique, «pourra d'ailleurs utiliser et perfectionner ses connaissances linguistiques». Le P. A. Bea, Recteur du Biblique a été consulté. Et le P. de Bonneville communique cette orientation à Lyonnet le 26 novembre 1935,

⁽⁶⁴⁾ L'étude parut: «Un important témoin du texte césarien de saint Marc: la version arménienne», *MUSJ* 19 (1935) 25-66, version arménienne de Mc 1,1-45, avec reconstitution du texte grec et notes critiques.

⁽⁶⁵⁾ C'est à Paray-le-Monial qu'elle eut ses révélations. Lagrange y fit allusion dans son commentaire de *L'Évangile selon saint Jean* (ÉB; Paris 1925) 528.

⁽⁶⁶⁾ ARSI, *Reg. Prov. Lugd.* xi, 418.

en ajoutant: «Quant à moi, mon cher Père, connaissant votre pensée comme vous connaissez la mienne, je n'ajoute qu'un mot sur le plan humain. Peu important les positions; elles valent ce que l'on y met».

Le 20 janvier 1936, Lyonnet prend contact avec le P. Bea: il songe toujours à éditer la version arménienne des évangiles et signale que «la reconstitution du modèle grec est achevée pour les six premiers chapitres (de Marc)»⁽⁶⁷⁾.

Durant l'année 1936, il n'y a pas d'autre lettre de Lagrange à Lyonnet. Le vieil exégète n'oublie pourtant pas son collaborateur. Ainsi, le 27 février 1936, écrivant à A. Condamine, son ami, il note: «Quel charme de traiter avec le R.P. Lyonnet! Je voudrais bien que le succès de tout le volume répondît à l'intérêt de ce qu'il nous a donné avec tant de bonne grâce»⁽⁶⁸⁾.

Plus encore, Lyonnet a rencontré Lagrange, probablement durant l'été, et lui aura fait part de sa nouvelle orientation vers l'Institut Biblique de Rome. Lagrange en parle dans une lettre du 6 octobre 1936 au T.R.P. Gillet, Maître Général des Frères Prêcheurs: «J'ai vu aussi le R.P. Lyonnet, S.J., mon collaborateur extrêmement sympathique pour la *Critique textuelle*. Je ne serais pas éloigné de penser que le fait a attiré sur lui la considération de ses supérieurs majeurs, puisqu'on l'appelle à Rome, où d'ailleurs les Français sont si peu représentés à l'Istituto Biblico»⁽⁶⁹⁾.

Arrivé à l'Institut Biblique de Rome, probablement dans le courant du mois d'octobre 1936, pour y prendre en deux ans les grades académiques en Écriture Sainte, Lyonnet écrit, vers la fin octobre ou au tout début novembre, une lettre au P. Ledóchowski: «Mon très révérend père, Votre Paternité me permettra de lui soumettre très simplement et filialement un ou deux faits nouveaux dont le R.P. Arnou et le R.P. Assistant m'ont conseillé de lui parler. L'an dernier au mois de novembre, mourait presque subitement un jeune arménisant de Paris, qui devait succéder à M. Macler, dans la chaire d'arménien à l'École Nationale des Langues orientales vivantes. Or, il y a quelques mois à peine, j'avais l'occasion de causer assez longuement avec M. Macler, et, à mon grand étonnement, il me demanda s'il ne pourrait pas compter sur moi pour le remplacer...». Le 4 novembre, le Père Général lui répond un petit mot de sa main: «Mon Révérend Père, Pax X, Après avoir examiné votre projet, il me semble qu'il est plus *ad maiorem Dei gloriam* que vous vous prépariez à enseigner le N.T. à l'Institut Biblique. J'espère que sur cette voie le bon Dieu vous permettra de rendre de grands services à l'Église. En union etc.»⁽⁷⁰⁾. Et c'est ainsi que Lyonnet devint l'exégète que l'on sait.

Le lendemain de ce billet, A. Bea écrit à M. Lobignac, Directeur de l'Institut Biblique à Jérusalem, : «Fra i biennisti c'è anche il P. Lyonnet, il quale è destinato per essere poi professore di esegesi del N. Test. qui all'Isti-

⁽⁶⁷⁾ Ces documents se trouvent aux APIBR; le dernier est daté par erreur de 1935.

⁽⁶⁸⁾ Archives des jésuites de France à Paris; copie aimablement communiquée, ainsi que la suivante, par B. Montagnes, O.P.

⁽⁶⁹⁾ AGOP.

⁽⁷⁰⁾ Ces deux documents, le premier en copie, se trouvent aux APIBR.

tuto (e forse un po' dell'armeno e giorgiano, quando il P. Zorell non potrà più). Il P. L. fa una ottima impressione, e credo che sarà un professore eccellente»⁽⁷¹⁾.

Lettre n° 13:

St-Maximin (Var), 8 janv. 1937

Mon cher Révérend Père,

J'aurais dû vous remercier du fascicule des *Mélanges de Beyrouth*⁽⁷²⁾, et vous augmentez ma confusion en me prévenant par l'expression de vos vœux. C'est une grande consolation pour moi que vous vouliez bien me recommander à Notre-Seigneur, et je ne manque pas à ce rendez-vous; je penserai surtout à vous le 2 février puisque vous devez ce jour-là prononcer vos derniers vœux sous les auspices de Notre-Dame, offrant à Dieu son divin Fils.

Permettez-moi de vous féliciter encore, comme je l'ai fait déjà, d'être appliqué par l'obéissance à l'étude du N. T., avec ses lumières et sa douceur intime.

S'il vous était possible d'étudier les leçons d'Origène et d'Eusèbe, sujet renouvelé par les dernières découvertes, je suis sûr que le Père Vincent en serait très heureux et très honoré pour la *Revue Biblique*⁽⁷³⁾. J'avais bien jeté un coup d'œil sur l'article de M. Ayuso⁽⁷⁴⁾, mais sans être en état d'en mesurer la portée. Une mise au point de votre part serait aussi très souhaitable. L'idée de deux sous-groupes dans le texte Césaréen paraît très indiquée. Il est très heureux que vous preniez rang, au nom des catholiques, parmi les spécialistes de critique textuelle appelés à remplacer v. Dobschütz et F. C. Burkitt. Le travail du P. Benoit sur P⁴⁶ m'a paru judicieux⁽⁷⁵⁾. Quelle énigme que ce ms. B! Mais ma confiance en lui n'est pas ébranlée dans la mesure que nous avons dite⁽⁷⁶⁾. Si vous venez en France à l'été, je serais bien heureux de vous rencontrer, au moins à Lyon. Mais fait-on des projets à mon âge?

Veuillez agréer, cher Révérend Père, l'expression de mon attachement respectueux en N. S.,

fmJ Lagrange
des fr. pr.

⁽⁷¹⁾ APIBJ.

⁽⁷²⁾ Cf. n. 64.

⁽⁷³⁾ Que je sache, Lyonnet n'a pas donné suite à cette suggestion.

⁽⁷⁴⁾ T. AYUSO, «¿Texto cesariense o precesariense? Su realidad y su trascendencia en la crítica textual del Nuevo Testamento», *Bib* 16 (1935) 369-415.

⁽⁷⁵⁾ P. BENOIT, «Le codex paulinien Chester Beatty», *RB* 46 (1937) 58-82, et dans sa conclusion: «le Papyrus... est... un témoin très fidèle du texte B» (81). D'où la réflexion de Lagrange.

⁽⁷⁶⁾ Cf. ce qu'écrivait C. M. MARTINI, «La problématique générale du texte de Matthieu», *L'Évangile selon s. Matthieu*, (ed. M. DIDIER) (BETL 29; Gembloux 1972) 35-36, et son recueil *La Parola di Dio alle origini della Chiesa* (AnBib 93; Roma 1980) 143-144, 120-121.

De fait, rentré en France à l'été, Lyonnet propose à Lagrange de le rencontrer à Lyon au début du mois d'août, si l'on comprend bien.

Lettre n° 14:

Roybon (Isère), chez M. Rambaud,
27 juillet 1937

Cher Révérend Père,

J'attendais pour répondre à votre bonne lettre de savoir si je ne pourrais pas vous joindre à Lyon. Malheureusement nos dates ne coïncident pas, et je ne puis les changer étant dans un engrenage de famille. Je puis aller passer *un jour* à Lyon ces jours-ci pour un baptême, mais ce sera probablement avant le 4 août, et j'y retournerai le 10 au soir, donc après votre départ pour Vienne. Je suis tout de même heureux d'avoir eu de vos bonnes nouvelles.

Le P. Vincent qui est ici avec moi me charge de vous dire combien il serait heureux d'imprimer dans la *Revue Biblique* tel article qu'il vous plairait, par exemple une réfutation de Baumstark⁽⁷⁷⁾, vraiment trop figé dans son Diatessaron, ou encore sur la version géorgienne, ou tel autre.

Je ne puis m'empêcher de vous dire que M. J. Guitton — votre cousin — est venu ici de St-Etienne en auto il y a deux ans, et retourné le même jour. La maison étant archi-comble, je ne puis malheureusement vous offrir une chambre, mais mon beau-frère⁽⁷⁸⁾ serait très heureux de vous offrir à déjeuner. Nous aurions plusieurs heures à causer. Si donc il vous était facile de trouver un chauffeur ami (qui nous ferait aussi le plaisir de déjeuner avec nous), je serais enchanté, et aussi le P. Vincent qui ferait votre connaissance. *Le mieux serait le plus tôt possible*⁽⁷⁹⁾.

Lyonnet aura-t-il trouvé ce chauffeur ami et répondu ainsi à la pressante invitation? La rencontre eut lieu plutôt à St-Maximin.

Lettre n° 15:

St-Maximin, le 6 janv. 1938

Mon Révérend et cher Père,

Je viens de relire attentivement votre lettre du 26 déc. Et d'abord merci de vos vœux. De mon côté, je demande à Notre-Seigneur de bénir vos études et de vous ramener à St-Maximin où je serais si heureux de vous revoir.

⁽⁷⁷⁾ Sur A. Baumstark, cf. LYONNET, *Bib* 19 (1938) 149, n.2, et *RB* 47 (1938) 379.

⁽⁷⁸⁾ M. Rambaud: cf. *supra*, n. 36.

⁽⁷⁹⁾ La lettre s'achève par ces mots: «Voici le schème grossier des routes» à partir de Marcilloles pour rejoindre la maison des Rambaud à Roybon; enfin: «Veuillez agréer mes sentiments respectueux en N.S.», et Lagrange oublia de signer!

Il m'est difficile de suivre votre raisonnement, dont je verrai les bases dans *Biblica*. Il me paraît cependant que vous avez reconnu l'existence d'une traduction arménienne du Diatessaron. Il n'y a rien là que de très vraisemblable. Vous ne dites pas si c'était d'après le grec ou le syriaque, mais vous semblez supposer un original syriaque. Ensuite on aurait fait une traduction des séparés⁽⁸⁰⁾ d'après le grec, et avec tant de soin qu'on ne se doutait plus de l'existence de la première version. Tout cela est du plus haut intérêt et fera honneur à vos recherches. Je ne puis prétendre en disputer les prémices à *Biblica*. Toutefois, permettez-moi de vous dire combien je désire qu'ayant collaboré aux *Études Bibliques*, qui font un avec la *Revue Biblique*, vous fassiez connaître aussi à ce même public les raisons que vous avez de modifier votre étude, c'est-à-dire de la compléter et de lui donner sa perfection.

Il me semble qu'il y a place et largement pour deux communications, la première plus générale intéressant tout le monde, la seconde adressée plus directement à vos premiers lecteurs, en prenant parti non pas contre vous-même, mais contre l'impression insuffisante qu'ils auraient pu se former⁽⁸¹⁾.

Au besoin on pourrait faire un tirage à part que les acquéreurs du gros volume pourraient lui annexer. C'est ce que je vous serais très reconnaissant de considérer, vous étant déjà très reconnaissant de votre contribution qui a été si appréciée. Il y aurait aussi à tenir compte des observations de Baumstark: il me semble que votre nouvelle thèse n'est pas en faveur de la sienne qui ne voit partout *que* le Diatessaron.

Veuillez agréer, mon très Révérend et très cher Père, l'expression de mon attachement respectueux in Christo Jesu et mes félicitations,

fMJ Lagrange
des fr. pr.

Ainsi s'achève cette correspondance qui révèle l'essor d'une étonnante amitié. Il n'est que de suivre, pour le percevoir, l'évolution des formules introductives et conclusives des lettres de Lagrange et son désir plusieurs fois exprimé de revoir Lyonnet. Celui-ci lui faisait part non seulement de ses recherches et travaux, mais aussi des grands moments de sa vie religieuse, et Lagrange y communiait avec toute sa délicatesse de croyant.

De St-Maximin, où il allait bientôt achever sa course, il écrivait encore, le 9 février 1938, dans une longue lettre au P. R. de Vaux, O.P., un de ses plus jeunes disciples, ce mot à propos de la *RB*: « Vous pouvez relancer le P. Lyonnet pour juillet »⁽⁸²⁾.

Le Père Lagrange s'éteignit le 10 mars. Répondant le 24 à la lettre de condoléances que Lyonnet lui avait envoyée, le P. L.-H. Vincent évoqua la

⁽⁸⁰⁾ C'est-à-dire les évangiles séparés.

⁽⁸¹⁾ Cf. n. 84.

⁽⁸²⁾ ASEJ, Dossier de la *RB*.

fin: «Tout a été si soudain! Le 1^{er} mars, le P. Lagrange revenait de Montpellier où il avait donné cinq ou six conférences à la jeunesse universitaire. Le 2, il commençait l'organisation de son travail pour le Carême. Le 3, il m'écrivait une longue lettre: questions d'étude, publications en cours, affaires de l'École, etc., terminée par une aspiration au repos et à la paix du ciel... que j'ai lue quelques heures après sa mort et sans la connaître encore! Le 4, il a fait son cours de Nouveau Testament — sur la Passion! — Ses dernières lettres sont du 8; et le 9, son état devenait tout à coup alarmant... pour ses frères qui l'entouraient, mais pas pour lui, car, averti, il a dit sans émoi ce mot qui reflète bien sa vie: 'Je m'abandonne à Dieu'. Le reste a été silence et calme...»⁽⁸³⁾.

Ainsi que le lui avait suggéré Lagrange, Lyonnet publia deux articles, l'un dans *Bib*, l'autre dans *RB*, la même année 1938⁽⁸⁴⁾. En tête du second, il exprimait sa «reconnaissance pour une affection dont nous restons profondément ému». L'article de *Bib* ajoutait à la dernière page un hommage au Père Lagrange, «laissant un magnifique exemple de droiture, de courage et de rare humilité».

Lyonnet allait poursuivre ses recherches arméniennes jusqu'en 1950 avec son livre *Les origines de la version arménienne et le Diatessaron*⁽⁸⁵⁾, qu'il dédiait à la mémoire de Lagrange, de Merk et de Zorell. Le lecteur peut estimer le poids de cette dédicace.

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⁽⁸³⁾ APIBR.

⁽⁸⁴⁾ «Vestiges d'un Diatessaron arménien», *Bib* 19 (1938) 121-150; «La première version arménienne des évangiles», *RB* 47 (1938) 355-382.

⁽⁸⁵⁾ *BibOr* 13; Rome 1950. Sur les travaux de Lyonnet concernant la version arménienne des évangiles, cf. B.O. KÜNZLE, *Das altarmenische Evangelium – L'Évangile arménien ancien* (Europäische Hochschulschriften XXI,33; Bern etc. 1984) 18*-30*.

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